THE

# AËRONAUTICAL JOURNAL.

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THE ORGAN OF THE ROYAL AËRONAUTICAL SOCIETY.

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*All communications should be addressed to the Editor.*

No. 129. SEPTEMBER, 1921. VOL. XXV.

## Notices of the Royal Aëronautical Society.

### Lectures.

In addition to the lectures announced last month, the programme for the next Session has now been completed by the addition of the following papers:—

Oct. 6.—"Some Notes on Aeroplanes in Tropical Countries," Air Commodore H. R. M. Brooke-Popham.

Oct. 20.—"The Langley Machine and the Hammondsport Trials," Mr. Griffith Brewer.

### Donations.

The Council desire to thank the British Aeroplane Company for their kind donation of a set of slides of the Bristol Ten-Seater Commercial Aeroplane for the Society's loan collection of lantern slides.

### Aeroplane Camera.

An inquiry has been received for an aeroplane camera which is wanted for experimental purposes. Any member who may have a suitable camera for disposal should communicate with Dr. E. P. Farrow, Limehurst, Spalding.

### Scottish Branch.

Details of the following arrangements have been received from the Honorary Secretary of the Scottish Branch:—

Sept. 19th.—Annual Meeting. The Right Hon. Lord Weir, LL.D., in the chair.

The proposed arrangements for Sir Keith and Sir Ross Smith's Cinema Travelogue in Hengler's Circus, Sauchiehall Street, on their "Flight to Australia," are as follows:—

Oct. 3rd.—Proposed luncheon by the Lord Provost and Magistrates, when the Lord Provost and Magistrates will be invited to the evening lecture.
Oct. 4th.—Dinner by the Royal Aëronautical Society at 6 p.m. Lecture under the auspices of the Society.
Oct. 5th.—Lecture under the auspices of the Glasgow Education Authority.
Oct. 6th.—Lecture under the auspices of the Royal Philosophical Society.
Oct. 7th.—Lecture under the auspices of the Royal Institute of Engineers and Shipbuilders.
Oct. 8th.—Students' and Boys' Brigade Officers' night.

LECTURE PROGRAMME.

Oct. 17th.—Lecture by Colonel V. C. Richmond (of the Airship Department, Directorate of Research, Air Ministry), on "The Organisation of a Colonial Airship Service," in the Engineering Class-Room of the University, at 8 p.m. *Chairman*—Lord Invernairn.

Oct. 31st.—Lecture by Professor Gordon Gray, D.Sc., of Glasgow University, on "Research Work on the Application of Gyroscopes to Aviation, with Solutions of the Problem of the True Vertical on Moving Vehicles," in the Natural Philosophy Class-Room of the University, at 8 p.m. *Chairman*—Mr. James Weir, C.M.G.

Nov. 14th.—Lecture by Major-General Sir Hugh Trenchard, Bt., K.C.B., C.B., Chief of Air Staff, on "The Auxiliary Air Force," in the Large Hall of the Institute of Shipbuilders and Engineers, Elmbank Crescent, at 8 p.m. *Chairman*—The Right Hon. Lord Weir, P.C., LL.D.

Dec. 12th.—Lecture by Colonel Gold, of the Meteorological Dept., Air Ministry, on "The Application of Meteorology to Aviation," in the Royal Technical College, George Street, at 8 p.m. *Chairman*—Sir John Hunter, K.B.E.

1922.

Jan. 23rd.—Lecture by Brig.-General R. K. Bagnall Wild, C.M.G., C.B.E., Director of Inspection, Air Ministry, on "Installation," in the Royal Technical College, George Street, at 8 p.m. *Chairman*—Sir John Reid, D.L.

Feb. 11th.—Lecture by Mr. H. Ricardo, A.M.Inst.C.E., on "Some Problems connected with Engines for High Altitude," in the Engineers' Class-Room of the University, at 8 p.m. *Chairman*—Professor Mellanby, D.Sc.

Feb. 25th.—Lecture by Mr. Allan E. L. Chorlton (Air Expert of Messrs. Beardmore, Ltd., London), on "Special Light Weight Engines." *Chairman*—Mr. Harold E. Yarrow, C.B.E.

Mar. 11th.—Lecture by Professor L. Bairstow, F.R.S., C.B.E., Professor of Aeronautics in the Imperial College, London, on "Natural Flight Instincts of some Modern Aeroplanes," in the Engineers' Class-Room of the University, at 8 p.m. *Chairman*—Professor Cormack, D.Sc.

Mar. 18th.—Lecture by Colonel Lockwood Marsh, O.B.E., LL.B., Secretary of the Royal Aëronautical Society, on "Some Side Lights on the History of Aeronautics," in the Royal Technical College, George Street, at 8 p.m. *Chairman*—Ex-Professor Barr, LL.D.

W. LOCKWOOD MARSH, *Secretary*.

## R38.

### August 24th, 1921.

The disaster to R38 is, without exception, the greatest single catastrophe in the history of aeronautics. The members of this Society who perished therein are commemorated elsewhere in this issue of the JOURNAL, but it is felt that the names of all who lost their lives should be recorded here. The relatives of each and all of them may rest assured that we mourn with them. It is perhaps not unfitting to express the hope that this disaster may not retard the future development of airships. It would be lamentable indeed were their sacrifice allowed to prove vain.

**British Officers.**

Air Commodore E. M. Maitland, C.M.G., D.S.O., A.F.C.
Flight Lieutenant J. E. M. Pritchard, O.B.E., A.F.C. (Air Ministry Representative).
Flight Lieutenant G. M. Thomas, D.F.C.
Flight Lieutenant I. C. Little, A.F.C.
Flight Lieutenant R. S. Montagu, D.S.C. (Navigator).
Flying Officer V. H. Wicks.
Flying Officer T. F. Matthewson, A.F.C. (Engineer Officer).

**National Physical Laboratory Representatives.**

Messrs. J. R. Pannell and C. W. Duffield.

**Royal Airship Works, Cardington, Representatives.**

Mr. C. I. R. Campbell, Superintendent, and Mr. F. Warren.

**British—Other Ranks.**

Flight Sergt. S. J. Heath.
,, ,, W. H. Greenel.
,, ,, H. Thompson.
,, ,, F. Smith.
,, ,, J. Rye.
,, ,, A. T. Martin.
Sergeant J. W. Mason.
,, F. E. Burton.
L.A.C. G. S. Anger.
L.A.C. W. Oliver.
L.A.C. J. N. Willson.
A.C.1 J. O. Drew.
A.C.1 C. W. Donald.
A.C.1 C. W. Penson.
A.C.1 E. E. Steere.
A.C.2 R. Parker.
A.C.2 R. Withington.

**American Officers.**

Commander L. A. H. Maxfield, U.S.N.
Lieutenant Commander W. N. Bieg, U.S.N.
Lieutenant Commander E. W. Coil, U.S.N.
Lieutenant H. W. Hoyt, U.S.N.
Lieutenant C. G. Little, U.S.N.
Lieutenant M. H. Esterly, U.S.N.

**Americans—Other Naval Ranks.**

C.M.M. L. E. Crowl.
C.M.M. A. L. Loftin.
C.M.M. W. A. Julius.
C.M.M. G. Welch.
C.B.M. C. J. Aller.
C.M.M. J. T. Hancock.
C.B.M. M. Lay.
C.M.M. R. M. Coons.
C.M.M. W. J. Steele.
C.B.M. A. D. Pettit.

*Per Ardua ad Astra.*

# Obituary.

## AIR COMMODORE E. M. MAITLAND, C.M.G., D.S.O., A.F.C., A.F.R.Aë.S.

Edward Maitland Maitland was born in 1880, the elder of two sons—of whom the younger, Harry, died in hospital during the war—of the late Arthur Maitland, Barrister-at-law, of Shudy Camps Park, Cambridgeshire. He was educated at Haileybury College and Trinity College, Cambridge, after which he came to London to read for the Bar. He, however, volunteered for service in South Africa, and on returning home received a permanent commission in the Essex Regiment. He took up ballooning about 1907 and received most of his early training from the late A. E. Gaudron, with whom and Mr. C. C. Turner he made a " record " balloon voyage from England to Russia in November, 1908, a distance of 1,117 miles being covered. Thereafter Maitland became one of the most ardent balloonists in this country, and, when he could be persuaded to relate his experiences, had a wonderful fund of anecdotes of adventures in balloons. His first parachute descent was made at the Alexandra Palace in 1908. In 1910 he purchased a Howard-Wright biplane, on which he proceeded to teach himself to fly, being one of the competitors at the Doncaster Aviation Meeting of 1910. This machine was subsequently purchased by the War Office, and he was very proud of the fact that it was the first aeroplane acquired by the British military authorities. Early in 1911 he had a bad crash in an aeroplane, in which both his ankles were broken, which necessitated a prolonged stay in hospital. On recovery he was, somewhat to his disappointment at the time, posted to No. 1 (Airship) Company of the Air Battalion at Aldershot, as he was not yet considered fit to fly an aeroplane. He would often refer, in later life, to the curiously fortuitous circumstances of his original connection with airships, of which he was to become the most devoted adherent and ardent exponent. His aeronaut's (balloon) certificate was numbered 13 and dated November 22, 1910, which was followed by his taking the British airship pilot's certificate No. 8 on September 19, 1911; while, although he first started flying aeroplanes much earlier, he did not take his aeroplane brevet until April 4, 1913, going to the Farman School at Etampes for the purpose and receiving French certificate No. 1, 281. On January 1, 1914, when the Army authorities ceased experimenting with airships, Maitland transferred as a Squadron Commander to the Naval Wing of the Royal Flying Corps, being then the senior Army airship officer, and became second-in-command to Commander E. A. D. Masterman, of the Naval Airship Service. Shortly prior to this, on October 18, 1913, he carried out the first parachute descent from an airship, the Beta, in flight. In the early summer of 1914 he went to Bitterfeld in Germany to watch the construction of a Parseval which had been ordered, but was never delivered, for the Navy. He only just succeeded in getting back to this country before war broke out, and almost immediately proceeded to Belgium and was chiefly instrumental in organising the balloon detachment which operated from Firminy; finally taking out the Beta in November, 1914. During this period Maitland became imbued with the importance of the kite-balloon, as used by the Germans and Belgians, and there is in the archives of the Admiralty his despatch which was the cause of the adoption of this weapon by the Royal Naval Air Service. It was therefore inevitable that he should be appointed to command the Kite-Balloon Training Station at Roehampton when this was commissioned in March, 1915. He remained there until the spring of 1916, during which time he carried out much experimental work, regardless of personal danger, including the first voluntary free run in a kite-balloon, to ascertain the effect of cable breakage, and an experimental parachute descent from a height of 10,000 feet. From Roe-

hampton he came to take charge of the Operations Section of the Air Department, Admiralty, but only remained there for a few months as his health would not stand unaccustomed office work. In the autumn of 1916 he was appointed commanding officer to commission Pulham, the first rigid airship station to be completed in this country. He was recalled from there to the Admiralty again in June, 1917, to take up the responsible duties of Captain Superintendent, Lighter than Air, in the Air Department. In November, 1917, he assumed the title of Superintendent of Airships when the Airship Section of the Air Department became the Airship Department, Admiralty, on the transfer of aeroplanes and seaplanes to the Air Board. This position he held until the signing of the Armistice, and for some time thereafter, and it was almost entirely due to the effect of his personality that the Board of Admiralty embarked upon a greatly increased programme of airship construction, which included twelve rigid airships and a large number of the non-rigid type. When airships were transferred to the Air Ministry at the end of 1919 he went to Kingsway in a somewhat indefinite appointment to assist in fitting the various portions of the Airship Department into the appropriate departments of the Air Ministry organisation. At the time of his death he was Commanding Officer of Howden Airship Base.

Though it is not possible to detail his many parachute descents here, it must not be forgotten that during all the period from 1916 onwards he took any and every opportunity of jumping off in a parachute from every type of aircraft—balloon, kite-balloon, airship and aeroplane—for no other reason than to give others a lead and prove to the authorities and the public that the parachute offers a means of saving life in case of accident. In view of this it is peculiarly significant that when found after the accident to R38 he was shown to have devoted his last moments to an endeavour to check the fall of the airship rather than to saving his life by means of a parachute.

It is impossible to write adequately of Maitland's personal charm of appearance, voice and character. Everyone who met him received the impression —which every action of his did nothing but confirm—of absolute honesty of purpose, combined with an unflinching devotion to the cause of airships. He imbued those who served with him with a sense of trust which was absolute, while the personal love which he inspired in all with whom he came in contact was truly wonderful. He had tact to a quite exceptional degree, and his manners recalled the courtly grace of a bygone age. To all those who knew him he will live as an unforgettable memory. He was a firm supporter of this Society, being elected as a Founder Member in April, 1909, and made an Associate Fellow in 1912. He served on the Council from March, 1912, to November, 1913. Many members will remember the delight which he gave to an audience of children, when he related his experiences during the Atlantic flight of R34 at the Society's Annual Juvenile Lecture in January, 1921, whilst his last opportunity of manifesting his interest in the Society was in very appropriately taking the chair on the occasion of Major Orde Lee's paper on "Parachutes" and Mr. Dyer's "Airship Fabrics" lecture on March 3 this year.

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## CONSTRUCTOR COMMANDER C. I. R. CAMPBELL, O.B.E., M.I.N.A., F.R.Aë.S., R.C.N.C.

Charles Ivor Rae Campbell was educated between the years 1894 and 1899 at the Royal Naval Engineering College, Devonport, where he obtained the Newman Memorial Prize for highest proficiency in engineering subjects. From there he went in 1899 to the Royal Naval College, Greenwich, where his career was remarkable, in that he followed the engineering course by taking also the course in design for constructors; thus obtaining very exceptional qualifications for his future career. On leaving Greenwich he went to the Admiralty, where

he remained until 1908, engaged chiefly upon submarine design work. He was then appointed overseer for submarine hulls at Messrs. Vickers' Works at Barrow-in-Furness, where he remained four years, during which period he laid the foundation of his future airship work through being able to watch the construction of Naval Airship No. 1, the first British rigid airship, which was built at Barrow while he was there. From Barrow he was appointed, in 1913, Admiralty overseer at Messrs. Armstrong's works at Newcastle-on-Tyne for the construction of H.M.S. Malaya and various submarines, where he remained until December, 1914, when he sailed for America as Admiralty overseer of the construction of submarines and M.L. boats in the United States. He also went to Canada, where he supervised the erection and testing of these boats before returning to England to undertake airship design work under Mr. A. W. Johns, in the Department of Naval Construction at the Admiralty, at the end of 1915; being immediately responsible for the work of the airship design staff then formed.

It was soon after this that he produced the design of the "23X" class of rigid airship. The design, of which only R27 and R29 were built, was approved in spite of considerable opposition owing to the absence of a structural keel, there being merely a corridor through the hull which was not a strength member. In 1917, on the reorganisation of airship work in the Admiralty, he assumed the primary responsibility for airship design by taking charge of the Design Section of the department of the Director of Production (Airships) under the Navy Controller. It was during this period that the design of R38, which incorporated many original features, was prepared. On leaving the Admiralty in 1920 for the Air Ministry, he became Superintendent of Airship Construction and Design at the Royal Airship Works, Cardington.

Campbell was unquestionably the most experienced and foremost airship designer in this country, and had, outside Germany, a unique knowledge of the problems of rigid airship design and construction. He had the fullest confidence in the future of airships, and was a firm believer in the importance of progressive increase in size. For the head of a department he assumed an unusual amount of personal responsibility for the work, and was never content to leave the smallest detail to others. He was always particularly anxious to study the results of his work in practical flight, and had been on all the trial flights of R38, and had succeeded in obtaining the somewhat reluctant permission of his superiors to his embarking in the airship when she should leave for America.

He had done much proselytising work to arouse both technical and public interest in airships, among the lectures he gave being one before the Institute of Naval Architects on "The Development of Airship Construction," on April 10th, 1919. He was elected an Associate Fellow of the Society on March 16th, 1920, which was followed by his election to Fellowship on April 19th this year. On April 25th last, at the request of the Society, he read a Paper on "Airship Transport," at the Olympia Efficiency Exhibition.

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## FLIGHT LIEUTENANT J. E. M. PRITCHARD, O.B.E., A.F.C., F.G.S., A.F.R.Aë.S.

John Edward Maddock Pritchard was born at Leighton Buzzard in 1889, and was of Welsh ancestry. He was educated privately and at Trinity College, Cambridge, whence he proceeded to the Royal School of Mining, South Kensington, where he graduated First Class in mining surveying. He then took up the career of a mining engineer, and was at one time engaged upon one of the most important mining surveys ever carried out in this country, having been elected a Fellow of the Geological Society of London some time previously. His interest in aeronautics was not confined to his war experience, as he had made several flights in the early days of aviation with, amongst others, the late S. F. Cody.

He joined the Royal Naval Air Service as a Flight Sub-Lieutenant on May 24th, 1915, and took a preliminary course in balloon piloting and aerostatics at the Kite Balloon Training Station at Roehampton. From there he proceeded to Kingsnorth R.N. Airship Station, in August of the same year, from where he was posted to Polegate as captain of an S.S. airship. He remained at Polegate until April, 1916, when he was sent out to the airship station at Mudros, where anti-submarine patrols were carried out in conjunction with the Allied Fleets. While in the Mediterranean Pritchard contracted dysentery, and was ordered home in consequence, being posted on his return again to Polegate as Senior Flying Officer. He was promoted Flight Lieutenant at the end of 1916, and in the following year took command of a coastal type airship (C24) at East Fortune. He subsequently was captain of a Parseval airship (P6) at Howden, where he achieved the reputation of being one of the most capable and scientific pilots in the airship service. On the formation of the Airship Department at the Admiralty, Pritchard was appointed, in September, 1917, to the staff, where he was responsible for the maintenance of and supply of spare parts for rigid airships, and also became acceptance pilot of rigid airships as these were taken over from the constructors by the Superintendent of Airships, representing the flying side of the service. On the formation of the Royal Air Force, on April 1st, 1918, he was appointed acting Major (S.O.2), having been promoted Flight Commander in the previous January. From then onwards perhaps his most important work, apart from his acceptance duties, was his collaboration with the Air Intelligence Department of Home Forces G.H.Q., at Whitehall, in extracting, from the information obtained during and after Zeppelin raids in this country, technical data of incalculable value as to the development of airship design and practice in Germany. In the course of this work he examined several batches of Zeppelin crews, on one occasion visiting France for the purpose of interrogating the crew of L49, and assisted in the translation of innumerable log-books and other documents which proved a mine of information, which was embodied in a series of reports the importance of which it is impossible to over-estimate. As the natural outcome of this experience, Pritchard was sent to Germany in December, 1918, as technical airship officer under Admiral Browning on the British Naval Section of the Inter-Allied Armistice Commission. He returned with a mass of further information, which he made available to others in his usual clear and concise manner. In July, 1919, Pritchard represented the Air Ministry on the Atlantic flight of R34, and on the arrival of the airship over the landing field at Long Island volunteered to land by parachute in order to take charge of the ground landing party in the absence of Major Fuller, who had proceeded to another emergency landing ground where it had been thought that R34 would be compelled to land through shortage of petrol. Since October 22nd, 1919, Pritchard had been in the Lighter-than-Air Section of the Air Ministry Research Department as acceptance pilot, receiving a short service commission as Flight Lieutenant on January 8th, 1920.

As a pilot he combined to an unusual extent flying ability with scientific knowledge, which made him one of the most valuable members of the airship service. He was particularly notable for tracing to its source any unusual phenomenon which might occur during the flight of an airship and for his grasp of the lines of development required for the improvement of airships. It may be said of all his work that he laboured indefatigably for the good of the airship service in general and was always anxious to make available for all any knowledge which he obtained. He was of an unusually attractive disposition; never too busy to give assistance or advice, always cheery, and a most amusing and enlivening companion.

Pritchard was elected an Associate Fellow of the Society on February 17th, 1920, and was a member of the Candidates and Library and Publications Committees. He lectured before the members on "Rigid Airships and Their Development" on February 4th, 1920.

## J. R. PANNELL, A.M.I.M.E., F.R.Aë.S.

John Robert Pannell received his technical education at the Northampton Institute, where he was awarded a diploma, after which he went through the shops of Messrs. Bruce Peebles, Edinburgh. He went to the National Physical Laboratory as a student assistant in the engineering department, in 1906, and was first employed in aiding Mr. Jakeman, who was conducting experiments on the specific heat of steam. After two years as a student, he was taken on to the Laboratory staff as a junior assistant, and helped Dr. T. E. Stanton in work on the strength and fatigue of welded joints, the results of which were published in their joint names in 1912. He then pursued a lengthy series of researches on the friction of fluid flow in pipes and the rate of heat transference from fluids flowing through pipes, and established experimentally the law of dynamic similarity for pipes by comparing the results obtained with air, water and oils. A joint Paper of Pannell's with Dr. Stanton was published in Vol. 214 of the "Philosophical Transactions of the Royal Society," at page 199, on "Similarity of Motion in Relation to the Surface Friction of Fluids." In 1914 he was transferred to the Aeronautics Department (then a branch of the Engineering Department) of the Laboratory, and since that date had been continuously employed there. His early aerodynamical work covered a wide field, among the subjects on which he was engaged being a systematic research on biplane systems; whilst he carried out tests on the model of the original Handley Page aeroplane. Later on during the war he took up the subject of resistance of bombs, and did much valuable work, in conjunction with Mr. N. R. Campbell, towards improving the technique of the measurement of resistance of stream-line bodies. This naturally led him to the subject of airships, with which he was almost exclusively concerned from 1917 onwards.

In this work on airship research Pannell was always very fully alive to the necessity of corroborating the results of his research work on models by measurements obtained in actual airships in flight, and was most emphatic in urging the importance of at least one airship being permanently detailed for this purpose, in order that research work might not be thrown back by the constant delays owing to airships being diverted to other work. He organised a very complete system of experiments from this point of view, and had made many experimental flights, notably in R33 and R36, as a result of which he collected a mass of most valuable data, often under most trying conditions. It would be impossible to overestimate the value of the work he did in this direction, which was carried out with characteristic enthusiasm and thoroughness, frequently at great personal inconvenience. At the time of his death he had practically completed a comparison of resistance of a stream-line shape in air and water, and was also engaged upon an investigation into the effect of surface roughness on airship resistance. It would be impossible to find a more enthusiastic believer in the commercial value of airships, in which he had great confidence, or a more ardent worker in the cause of airship development.

Pannell was elected an Associate Fellow of the Society on April 19th, 1917, and a Fellow on July 5th, 1918. He was a member of the Sub-Committee on Symbols of the Technical Terms Committee, 1919, and in 1917 read a Paper to members on "The Wind Channel: Its Design and Use."

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## LIEUTENANT C. G. LITTLE, U.S.N.R.F.

Lieutenant Charles G. Little was born at Newburyport, Massachusetts, in 1895, and entered the United States Service of Naval Aviation on May 9th, 1917, as an Ensign, being promoted Lieutenant "J.G." in 1918, and full Lieutenant later in the same year. He was one of the American Naval Aviation officers who

came over to Europe as airship pilots during the war, being stationed at Paimbœuf Air Station in France, and held the French airship pilot's brevet, No. 97. He was awarded the U.S. Navy Cross for his services. Lieutenant Little joined the Society as a Foreign Member on October 19th, 1920.

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## LIEUTENANT-GENERAL SIR DAVID HENDERSON, K.C.B., K.C.V.O., D.S.O., Hon.F.R.Aë.S.

"David" has gone, and the world of aviation laments the departure of one of its most interesting and greatest figures.

David Henderson started life as an engineer, but opportunity offered and he transferred his activities to the more honourable and less lucrative profession of arms. His career as a soldier was conspicuously successful, and when aviation first appeared on the military horizon, he had a long and distinguished record of active service to his credit. His greatest successes had been won in the Intelligence Branch of the General Staff—and it was this experience which qualified him particularly to grasp the vast possibilities of aircraft as a means of reconnaissance when his military and naval contemporaries were both sceptical and inert in their attitude towards this new weapon of war.

In 1912 he stepped into the aeronautical world by "taking his ticket" at Brooklands at the age of 50, to the furtive annoyance of some of his equals and to the amazement and admiration of his juniors; at the time he must have been the oldest man in the world who had attained this qualification. General Henderson was then Director of Military Training in the War Office, and it was probably the fact that he was the only senior officer in the Army who could fly, which led to military aviation being placed under a branch of his particular directorate.

From that moment his whole life was devoted to the new cause. No one who did not actually work under him can realise the vast amount of time and energy he gave to aviation during 1912 and 1913. The onerous duties of Director of the Training of the Army already more than fully occupied his time, but he spent untold hours both by day and night in tackling the endless and difficult questions which arose during the birth and development of the Royal Flying Corps and the Military Aeronautics Directorate. His foresight and judgment at this time were remarkable; if the archives of the War Office dealing with aviation at this period are ever seriously studied, they will be a revelation of his sound judgment combined with imagination and enthusiasm. Every step taken was considered fully and thoroughly, and the fruits of almost every decision of importance have endured to the present day through all the changes of control and of headquarter organisation which have occurred since. To him must be given the credit of the first conception of a flying service common to both Army and Navy, divided into Military and Naval Wings and controlled by an Air Committee, consisting of the leading officials in the Government and in the two great warlike departments concerned with aviation. This Committee had operated with great success for two years when the war started; if it could only have been continued with increased powers, much useless and pernicious overlapping and competition between the Admiralty and War Office could have been avoided. But David Henderson and some of the other members went off to the war, and the Committee was allowed to lapse, until the two Air Boards and eventually the Air Ministry restored the policy of central control over all matters concerned with aviation. For the first year of the war General Henderson commanded the Royal Flying Corps in the field with conspicuous distinction; Lord French, the Commander-in-Chief, was lavish in his praise of the good work he did, and it was under him that the Royal Flying Corps created a reputation for dash, courage, good discipline and reliability in battle, which was maintained in face of all difficulties until the last day of the war.

In the middle of 1915 David Henderson returned to the War Office and again took up the duties of Director-General of Military Aeronautics and Aviation Member of the Army Council, the post which he held before the war, and continued in this capacity until the autumn of 1917. During this period the Military Aeronautics Directorate and the Royal Flying Corps grew both in size and in accomplishment far beyond the dreams of the saner enthusiasts at the beginning of the great struggle. His counsels on the first Air Board under Lord Curzon in 1916 and the second Air Board under Lord Cowdray in 1917 were of infinite value, and were a potent factor in the steady development towards the creation of the Royal Air Force under an independent Air Ministry. By the frankness of his statements and his almost furious opposition to the machinations of self-seekers and self-advertisers, he earned for himself a not inconsiderable group of enemies and detractors, by whose instigation various criticisms were levelled at him in Parliament, in the Press and in private. They carried no weight, however, with those who really knew him, and in the Pemberton-Billing inquiry, held by the Government in 1916, he absolutely vindicated his policy on every debatable point, and established a reputation for clear-mindedness and ability in legal debate such as has seldom been attained before by a simple soldier.

In the autumn of 1917 it was decided to create an independent Air Ministry, and General Henderson was relieved of his appointment as D.G.M.A. and Member of the Army Council to collaborate with General Smuts and advise the War Cabinet as to the best lines of development and organisation.

In January, 1918, the Air Ministry came into being, and General Henderson was appointed a member of the Air Council.

Then came perhaps the worst moment in the whole history of the development of the flying services of this country. Lord Rothermere had been appointed as the first Air Minister; his inexperience in the workings of a Government Department and the difficulties of control of an entirely new and far from homogeneous service, led to serious differences of opinion between him and the Chief of the Air Staff, General Trenchard. The latter thought it his duty to resign, but this fact was not disclosed until a month afterwards, in March, 1918, when Lord Rothermere announced the fact and nominated his successor without consulting the Members of the Air Council.

General Henderson rose in righteous indignation and objected vehemently to the course adopted by the Air Minister and to his selection, and finally resigned as a protest against the policy pursued.

Very soon afterwards Lord Rothermere was forced to resign himself, owing to the difficulties of the situation he had created, but unfortunately David Henderson had gone, and never again took any active participation in the control of the Royal Air Force.

His departure was a great and irreparable loss to the young service, and the rectitude of his action during the crisis has been fully vindicated by subsequent events.

In the summer of 1918 he suffered a cruel blow from the death of his gallant son Ian, in an aeroplane accident in Scotland.

After his resignation from the Air Council, General Henderson served with distinction under the War Office for the few remaining months of the war, and then took over the control of the International Red Cross organisation at Geneva. But a reserved and highly sensitive temperament had been severely taxed by unremitting hard work in the service of his country for many years, and the resulting wear and tear, combined with the effects of his son's tragic death, must have undermined his health and cut short his career of usefulness and good service to the British Empire and to humanity at large.

David Henderson was a natural soldier, a great gentleman and a good Scotsman; a fighting enemy and an unflinching friend. He combined great ambition with devotion to duty, and fierce courage with gentleness and an exceeding kind heart. His fine spirit was remarkable, even amongst the most gallant of the old

Army; a quality which unfortunately gave him the power to undermine his health by overwork in the cause of aviation.

He was a delightful companion and a gifted raconteur. His subtle wit and imperturbable charm of manner captivated all those who came into contact with him, and his friends and admirers were legion. His scientific and engineering knowledge was remarkable for a soldier, and the Committee of Aeronautical Research owe much to his advice in the early days. He had great artistic talent and was a devoted lover of music.

Although he was a great soldier, he might well have been an even greater diplomat if Fate had led his steps into that walk of life; he was cautious in council and slow in deliberation, always manœuvring round a difficulty if it were humanly possible; but once it was clear that further consideration was useless, his action was swift, decisive and far-reaching.

Let us mourn seriously and sincerely for David Henderson; the rising generation does not promise to produce more men of his stamp and quality for the service of aviation. He has left us, but his works will live and grow and multiply; and the Air Force of the future should look back to him as their real Creator and their first Chief.

He had been an Associate Fellow of the Society since 1912, and was elected Honorary Fellow on December 14th, 1917.

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## SQUADRON LEADER G. H. NORMAN.

The progress of experimental aeronautics has suffered a serious loss by the death of Squadron Leader G. H. Norman, the head of the Engine Research Department of the Royal Aircraft Establishment, who died on 18th August, at the Cambridge Hospital, Aldershot. Squadron Leader Norman had been intended for the Royal Navy and educated at the Royal Naval Academy, Gosport, but afterwards studied at the Royal School of Mines and obtained an associateship. He was also an associate member of the Institution of Civil Engineers and the Institution of Mechanical Engineers and a B.Sc. (Eng.), London. In the early days of the war he served in the Artillery, but was transferred to the R.F.C., where he soon qualified as pilot, and spent thirteen months on the Western Front as Flying Officer and Flight Commander. It was whilst fighting in France that he conceived, made and used what became the standard aerial gun-sight for both the British and American forces.

This sight, which bears his name, was adopted without serious rival during the rest of war, its principal merit being provision of the absolute necessary allowances for the speed of contending aircraft, with the minimum of complications, being thus in marked contrast to other types of sight under trial at the time, most of these being far too complicated for general use in air fighting. After being wounded in action and twice mentioned in despatches, Squadron Leader G. H. Norman was posted to the Armament Station at Orfordness by the late Major Hopkinson, who, impressed by his great constructive ability and enthusiasm for work, placed him in charge of flying at the Experimental Station. From that time to the day of his last short illness his vigorous prosecution of every kind of experimental work was unceasing.

Though officially concerned only with flying at Orfordness, his advice was asked and generally taken in questions connected with almost every branch of the work of the station, and the influence of his clear thinking and great designing ability remains in many schemes and productions with which his name has been in no way connected, for his interest always lay in the work itself. He was by no means a typical inventor in the sense in which the word is popularly understood, that is to say, a man who works by brain waves without labour, his produc-

tions were always the result of careful thought and still more careful experiment. In 1918, when Major Hopkinson took over the Royal Aircraft Establishment, he placed Squadron Leader G. H. Norman in control of the Engine Research Department. As at Orfordness his interests were many and varied and were not confined entirely to engines, but it is probable he will be best remembered by his efforts to reduce the risk of fire in the air or on crash, which he maintained could be almost certainly avoided, given sufficient care in the arrangement of the aeroplane.

Perhaps Squadron Leader Norman's most striking characteristic, and one which effectively endeared him to all who worked with him and under him, was a complete disregard of personal danger, coupled with extreme care for the safety of all under his command. Some very urgent reason was required if he were not to be the first to carry out in person any dangerous experiment which he desired to make. He left behind him a supreme example of this in a flying experiment which he made a few weeks before his death.

An aeroplane had been fitted with an arrangement for spraying fire extinguishing liquid into the engine to extinguish fire in the air. So strongly did he feel the need of personal demonstration that he twice produced fire artificially round his engine in full flight and twice extinguished it.

Perhaps it is to those who fly that an act such as this will make the most intense appeal, but its significance will not end there, it will be remembered as long as the awful possibility of fire in the air remains.

Squadron Leader Norman was a valued member of the Society's Safety and Economy Committee, 1920, and was nominated to serve on the Candidates' Committee this year.

# SCOTTISH BRANCH.

## SECOND ANNUAL REPORT.

The Executive Committee have pleasure in submitting the second annual report of the activities of the Scottish Branch of the Society.

It is with the deepest regret that they have to record the death of two of their most active and valued members, Colonel Smith Park, D.L., M.V.O., and Mr. Jas. S. Nicholson, D.Sc. Colonel Smith Park was convener of the Finance Committee and a member of the Propaganda and Education Committee, and in spite of his many and varied activities he always found time to help and further the interests of the Society by his prudent and far-seeing counsel.

Mr. Jas. Nicholson, of Glasgow University, was an Associate Fellow and one of the few original members of the Society taken over by the Scottish Branch when it was formed. He was a great aeronautical scientist and a member of our Technical Committee. The Society in particular and aeronautics in general have sustained a great loss by his early death.

It is with great pleasure that we have to record that our Chairman has been raised to the Peerage. The news of this well-deserved honour was received with much gratification by the Executive, which has been duly expressed in their minutes.

Our Society continues to fill a place of considerable usefulness in regard to the cause of aeronautics in Scotland.

1. The Propaganda and Education Committee report that following on last year's success, a course of lectures was again arranged for the engineering students in the Scottish Universities of Glasgow, Edinburgh and Dundee, of which the following are particulars:—

Nov. 1st.—Sq. Leader J. S. Buchanan, R.A.F., from the Directorate of Research Dept. of the Air Ministry, on "Aircraft: General Design."
Nov. 3rd.—Sq. Leader Buchanan on "Aircraft: Details of Construction."
Nov. 15th.—Sq. Leader Hill, R.A.F., from the Royal Aircraft Establishment, Farnborough, on "The Technique of Flight."
Nov. 16th.—Sq. Leader Hill, second lecture, on "The Technique of Flight."
Nov. 22nd.—Mr. J. L. Bartlett, R.N., Chief Overseer, Inchinnan, on "Airships: General Principles."
Nov. 23rd.—Mr. J. L. Bartlett, on "Rigid Airships: Design and Materials."

The attendance at these was good, and it is of note that a course of aviation for the B.Sc. Degree has been instituted at the Glasgow University. The Society is indebted to Professor Cormack for his valuable help in this connection.

With the help of Professor Mellanby, to whom we tender our thanks, a large number of technical college students were interested in the course of aeronautics, and the Hon. Secretary addressed a large meeting of these in October last.

Feeling that they could not otherwise do full justice to the ever-increasing interest shown by the students (many of whom are ex-airmen), the Propaganda and Education Committee decided to form an Ex-Airmen's and Students' Section, and sufficient success has been achieved to show that there is every prospect in the future of useful work being done by the section when the time is ripe.

Good work has been done amongst the cadets of the Glasgow Public Schools and the usual annual Juvenile Lecture was given in March, 1921, by General Maitland on "Airships and their Future." Excellent lantern slides were shown

and great interest was evinced. There was a good attendance at this meeting, which again filled the large hall of the Engineers' Institute.

2. The general lectures held in the Institute of Engineers and Shipbuilders were as follows:—

Oct. 18th.—Major-General Sir Sefton Brancker, on "Aerial Transport." The Right Hon. Lord Weir of Eastwood presiding.

Nov. 8th.—Captain Jones, on "Aerial Photography." Mr. Buyers Black presiding.

Nov. 18th.—Air Commodore Brooke-Popham, C.B., C.M.G., D.S.O., A.F.C., Director of Research of Air Ministry, on "Lines of Future Progress in Aeronautical Research." Lord Invernairn of Strathnairn presiding.

Dec. 2nd.—Mr. C. V. Wallace, Inchinnan, on "Airships and their Future." Colonel Smith Park presiding.

Dec. 15th.—Lieut.-Colonel Strain, Edinburgh, on "Aeroplane *v.* Submarine." Sir John Reid presiding.

Jan. 26th, 1921.—Brig.-General Guy Livingston, C.M.G., on "The Future of Aviation." Mr. George F. Luke presiding.

Feb. 17th.—Major-General Sir Fred. Sykes, G.B.E., K.C.B., C.M.G., on "Developments of Civil Aviation." Lord Invernairn presiding.

Feb. 21st.—In the University, Sq. Leader Arnold J. Miley, O.B.E., on "Seaplanes and Flying Boats." Professor Cormack presiding.

Mar. 10th.—In the Large Hall of the Technical College, Major W. T. Blake on "Flying for Business and Pleasure." Professor Cormack presiding.

Mar. 14th.—In Engineers' Institute, to Cadets of Glasgow Public Schools, Air Commodore Maitland, C.M.G., D.S.O., A.F.C., on "Airships and their Future."

It is interesting to note that the lecture by Lieut.-Colonel Strain, D.S.C., O.B.E., R.A.F., "Aeroplane *v.* Submarine," was printed in the London "Times," and has now been reproduced in the JOURNAL.

A specially noteworthy lecture was that on "The Future of Aviation," by Sir Frederick Sykes, who was entertained at dinner, prior to the lecture, by the Hon. Secretary, when a number of distinguished citizens were invited to meet him.

The lectures were in all cases well attended, and in more than one instance the Large Hall of the Institute was filled. This encourages the Propaganda and Education Committee to continue their policy of getting the very best lecturers possible so as to keep members fully up-to-date in the latest developments of aeronautical science and research.

3. The activities during the year in regard to a most important matter, viz., the future of the Renfrew and Inchinnan Aerodromes, have been considerable, and several interviews took place with the Ministry of Munitions Disposals Board in regard to these. Ultimately the Air Ministry announced that the Renfrew Reception Park alone would be retained, and this on a five years' lease. At Inchinnan only the ground on which Messrs. Beardmore's works stand would be retained, and disposed of along with the Airship Construction Buildings. The Ministry has requested the co-operation of the Society with regard to the future of the Renfrew Reception Park, so that the various local authorities should, by this Society, be co-ordinated, and thus be in a position to co-operate with the Ministry in securing this park as a permanent home of aviation in the West of Scotland. It is intended that the movement in regard to securing the necessary co-ordination of all local parties interested, will be proceeded with in the near future.

4. The Executive have also been interested in the proposal regarding the formation of a Territorial Air Force. It is understood, in connection with the

institution of this force, that Glasgow will be one of the centres in which it is intended to move actively in the near future. Meantime, the Society has compiled a register of the ex-airmen of the district with a note of their flying record, and this will be of great assistance in the event of the Territorial Air Force being formed.

5. During the year negotiations were conducted with the Scottish Aeronautical Society so as to avoid any possible clashing of interests. The line of demarcation was arranged to be the same as that existing between our Society in London and the Royal Aero Club, viz., we to look after the scientific and technical interests of aeronautics, while the S.A.C. look after the social and sporting interests.

6. Activities in the Edinburgh and Dundee branches have been curtailed owing to difficult local conditions, and in Edinburgh, owing to our local Secretary having been called to London. Nevertheless, thanks to the great help of Professor Hudson Beare in Edinburgh and of Professor Fulton in Dundee, the interests of aeronautics have been kept alive, and it is intended, during the coming year, to increase activities in these branches by rendering every assistance possible from Glasgow, both in the way of lecturers and also by visits of members of our Executive to organise meetings, etc.

7. The Finance Committee report that the position in regard to finance is satisfactory, as will be seen from the statement attached. The annual subscriptions were raised by head office to the extent of about 50 per cent. during the year, mainly owing to the increased cost of printing, paper, etc. This has prejudicially affected the membership. But although the membership has been reduced the annual subscriptions still amount to the satisfactory sum of £231. The cash in hand at 31st May amounted to £30, after 50 per cent. of the receipts from subscriptions had been forwarded to the head office. The amount in hand of the Initial Establishment Fund was £500.

It is hoped that in the coming year every member will make a special effort to secure one other member so that the interest in aeronautics may be extended as widely as possible in Scotland and the financial position of the Society consolidated.

INVERNAIRN, *Chairman.*

J. BUYERS BLACK, *Hon. Secretary.*

# ROYAL AERONAUTICAL

## ABSTRACT OF ACCOUNTS FOR

### RECEIPTS.

| | £ | s. | d. | £ | s. | d. |
|---|---|---|---|---|---|---|
| CASH ON HAND at 1st June, 1920 ... ... ... ... | | | | 775 | 16 | 4 |
| ORDINARY— | | | | | | |
| Entrance Fees and Annual Subscriptions received from 1st June, 1920, to 31st May, 1921 ... | | | | 231 | 1 | 0 |
| EXTRAORDINARY— | | | | | | |
| Special Donations ... ... ... ... ... | £5 | 5 | 0 | | | |
| Special Subscriptions for Initial Establishment Fund | 75 | 0 | 0 | | | |
| Grant from Glasgow University towards cost of Scientific Course of Lectures on Aeronautics in Glasgow University ... ... ... ... | 25 | 0 | 0 | | | |
| Grant from Edinburgh University ... ... ... | 25 | 0 | 0 | | | |
| Grant from Dundee University ... ... ... | 25 | 0 | 0 | | | |
| One half of Dundee Subscriptions received ... ... | 3 | 13 | 6 | | | |
| Interest on Deposit Receipts ... ... ... ... | 14 | 16 | 9 | | | |
| Interest on Glasgow Corporation Loans ... ... | 10 | 19 | 0 | | | |
| Interest on Glasgow Corporation Deposit Receipt... | 3 | 18 | 1 | | | |
| | | | | 188 | 12 | 4 |
| | | | | £1195 | 9 | 8 |

GLASGOW, *21st June*, 1921.—Audited and found correct.

## SOCIETY (SCOTTISH BRANCH).

### THE YEAR ENDED 31st MAY, 1921.

EXPENDITURE.

| | £ s. d. | £ s. d. |
|---|---|---|
| ORDINARY— | | |
| Amount paid Head Office, London, being one half of Annual Subscriptions received by this Branch from 31st March, 1920, to 31st May, 1921, as per arrangement ... ... ... ... ... | 141 0 6 | |
| Miscellaneous Expenses of Lectures ... ... | 125 13 1 | |
| Printing ... ... ... ... ... ... ... | 19 12 6 | |
| Typewriting Supplies, Stationery, etc. ... ... | 23 17 5 | |
| Postages, Telegrams and Trunk Calls ... ... | 23 12 6 | |
| Clerical Assistance for year ... ... ... ... | 102 0 0 | |
| Advertising ... ... ... ... ... ... | 22 7 6 | |
| | | 458 3 6 |
| EXTRAORDINARY— | | |
| Hon. Secretary's Travelling Expenses, *re* Meeting Air Ministry and Minister of Munitions ... | 9 16 6 | |
| Paid for use of Office and Staff, Typewriters and Sundry Expenses from 31st August, 1919, to 31st May, 1921 ... ... ... ... ... | 196 19 2 | |
| Bank Interest ... ... ... ... ... ... | 1 0 0 | |
| | | 207 15 8 |
| BALANCE: Surplus of Receipts over Expenditure made up as follows:— | | |
| Cash in Bank ... ... ... ... ... ... | 43 3 4 | |
| Cash with Corporation of Glasgow, on loan ... | 500 0 0 | |
| | 543 3 4 | |
| *Less*: Cash due Secretary ... ... ... ... | 13 12 10 | |
| | | 529 10 6 |
| | | £1195 9 8 |

(Sgd.) J. WYLLIE GUILD & BALLANTINE, Chartered Accountants, *Auditors*.

# THE WILBUR WRIGHT LECTURE.

## SCIENTIFIC METHODS IN AERONAUTICS.

BY G. I. TAYLOR, F.R.S.

### Introduction.

When your Secretary, shortly before Easter, wrote to ask me to give the Wilbur Wright Lecture he made me give him a title before writing the lecture. I therefore had to think of one which would fit any lecture I might conceivably want to give, so that when I came back after Easter I should be able to pick out one of those subjects connected with aeronautics with which I have been specially concerned, and discuss the results arrived at. When, however, I came to sit down to write the lecture itself it struck me that it might be more interesting to a general audience if I followed the line which is perhaps most obviously indicated by my title, and directed my remarks towards tracing and comparing some of the principal lines of argument which have been used in aeronautical research, without going particularly deeply into the results.

When one comes to review the whole range of experiments and theories in aeronautics one is apt, if one is cursed with a passion for arranging things, to set up a series of files headed Propellers, Aerofoils, Engines, etc. This classification, which is no doubt the most useful one for the engineer, is unsuitable for the purpose I have in mind this evening. For this purpose I shall imagine aeronautical knowledge to be arranged roughly in a single series according to the number of links which intervene between the experiment or theory concerned and its application to aeronautical practice.

At the beginning of the series come direct tests in the air on the final product, tests of the speed and stability of an aeroplane, the flight of a bomb or the durability of a balloon. Next come tests of engines on test beds. Then the series will pass through various kinds of knowledge derived from experiments with models. Next will come stability calculations in which the results of model experiments are used, and finally the series will end among the fundamental principles of aerodynamics and hydrodynamics.

I propose this evening to pass along this series picking out some of the methods of solving aeronautical problems which have fallen within my experience, by way of illustration of the principles involved. These examples will, I hope, serve to throw into prominence the comparison which I want you to make between the methods of applied and those of pure science.

The worker in applied science can apply more or less direct methods which lead him continually nearer to his goal. The pure scientist has such formidable difficulties to encounter that direct methods are usually impossible. He must apply quite indirect methods, which constantly carry him into other fields of research.

The differences in the methods of applied and pure scientific research naturally give rise to differences in the conditions most suitable for carrying them out. These again may give rise to differences in the kinds of organisation which are likely to be of most assistance in furthering the two kinds of research. It is in the hope that I may be able to convey to people who are not themselves engaged in research some idea of its methods, some idea of its difficulties and some idea of the conditions which are most favourable for carrying it out, that I have embarked on my subject this evening.

### Full-Scale Experiments.

Returning now to the series into which we may imagine aeronautical knowledge to be arranged, we begin with direct tests in the air.

The only general scientific principle which appears to be involved in passing

from these tests to the performance of aeroplanes lies in the assumption that the same results always follow from a given sequence of causes. All of us, I suppose, are willing to admit this principle in our calmer moments, though I must confess to moments of exasperation when experiments went wrong and I was almost ready to deny it.

As an example of full-scale research one might be expected to select some method of dealing with the problems which occur in full-scale research on aeroplanes. On the other hand this Society has had so many accounts of these methods that it seems preferable to select an example in which experiments were made on the aerodynamic behaviour of some other body than an aeroplane. The body I have in mind is the flechette or aeroplane dart, which appeared and disappeared in the early stages of the war.

Some time in August, 1914, the War Office sent down to the Royal Aircraft Factory at Farnborough, where I was then stationed, a steel dart of French manufacture which was intended to be dropped from aeroplanes on to troops. They asked whether the R.A.F. could suggest any improvements. Mr. Busk, who was then in charge of the experimental side of the R.A.F., turned the job over to Professor Jones, who, after many experiments, finally decided that a dart consisting of a short, pointed steel rod forced into a piece of thin brass radiator tube might give better results and be cheaper to manufacture than the French all-steel dart. The two darts are shown in Fig. 1.

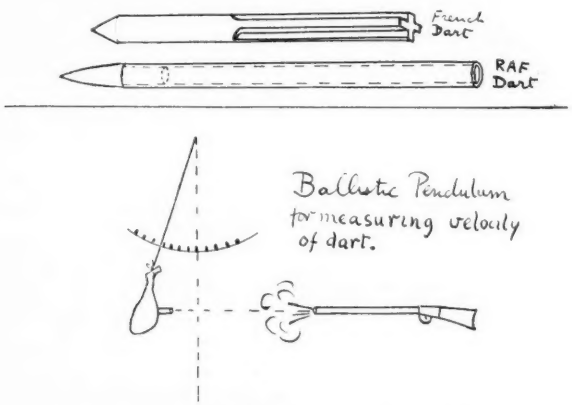


FIG. 1.—*Experiments on aeroplane darts.*

Having designed the dart, the next thing was to test its performance in the air, and it was at this stage that I became connected with the experiments. The first point to decide was the proper length to make the radiator tube. With this end in view we repaired to Pyestock chimney, a disused factory chimney 130 feet high, near Farnborough, and dropped darts with various lengths of tail tube down the middle. At first we had supposed that if a dart were made so that it would point into the wind when suspended horizontally at its centre of gravity, it would fall straight. This, however, turned out to be untrue. Unless the tail tube were considerably longer than the length necessary for stability when suspended at the centre of gravity, the dart would whirl round about a vertical axis, lying out to some 50 or 60 degrees from the vertical. When, however, the tail tube was made some $4\frac{1}{2}$ or 5 inches long the darts were found to fall quite straight down the chimney.

The next thing to find out was whether these darts would do any damage when they fell from an aeroplane on to troops. Direct experiments were impossible, partly owing to lack of volunteers to act the part of troops, and partly owing to the difficulty of hitting a target from an aeroplane in flight. It was therefore necessary to have recourse to indirect methods, though you will notice that the object experimented on was still the dart which it was intended to use in actual warfare.

The most obvious indirect method appeared to be to determine first the velocity with which the dart would arrive at the ground when dropped from an aeroplane, and then to project it with that velocity at the material, the penetration into which was to be tested.

To determine the velocity with which the dart would reach the ground it was merely necessary to determine its resistance. Quite a rough determination was sufficient, and this was obtained by dropping the dart, point downwards, through a stream of water which was rising in a tube. The speed of the upward current of water was adjusted till the dart would only just fall through it. The resistance was then equal to the weight of the dart.

From this experiment it was possible to make a rough estimate of the speed with which the dart would reach the ground. The next step was to find a method of projecting it at that speed. This was done by means of a Service rifle, bored smooth so that the dart would just fit it, and fired with a reduced charge. In order to find out how much charge was necessary, the dart was fired into a ballistic pendulum, which consists, as you probably know, of a heavy body hanging on a string. It was possible to calculate the velocity of the dart from the distance to which the pendulum swung out from its position of equilibrium. Conversely it was possible to calculate how far the pendulum should swing when struck by the dart at the velocity with which we had found that it would reach the ground. This calculation was made and the charge in the cartridge behind the dart was reduced till the pendulum was observed to swing through this distance.

As one of the questions we were asked was what would be the penetration of these darts into flesh and bone, we made our ballistic pendulum out of a leg of mutton, and so did our penetration experiments at the same time as the experiments to find the amount of charge necessary to propel the dart at the required speed. The sketch in Fig. 1 gives an idea of the experiment.

Having satisfied ourselves that the darts would do the necessary amount of damage if they struck their target, it remained to find out what chance they had of hitting that target.

For this purpose direct experiments were possible. Mr. Busk took a box containing a few hundred darts and dropped them from an aeroplane flying at about 500 feet above the aerodrome. Professor Jones and I then went carefully over the ground threading a little square of paper over each dart as we found it sticking in the ground, to mark its position. In this way we were able to make an estimate of the chances of hitting troops in any given formation. They turned out to be extremely small.

In connection with these experiments I remember a curious incident. While Professor Jones and I were standing looking at the scene which consisted of hundreds of little pieces of paper each transfixed by a dart, a highly polished cavalry officer came up on his horse and asked us what we were doing. We told him we were marking for an aeroplane which was dropping darts. His only comment was "Well, if I hadn't seen it, I should never have believed you could make such accurate shooting from an aeroplane." I leave you to trace his simple thought.

Aeroplane darts were practically never used by British pilots in warfare. There is of course a very adequate reason for this. The danger line of a

dart is a vertical line. For troops spread over the ground, therefore, each dart gives rise to a single danger spot. If, however, the same weight of material were used to make bombs, some of the danger lines of the pieces would be horizontal lines and the chance of hitting troops would be enormously greater. It is interesting to notice however that this was not the reason given at the time. We were told that British pilots refused to use darts because they regarded them as inhuman weapons. Pilots—and I speak as a pilot myself—are a most conservative race. Gunpowder as a means of propelling projectiles had acquired respectability through centuries of use, but gravity was a new and inhuman force. Bombs at that time were regarded as hardly respectable. Presumably the explosion at the end of their flight made up to a certain extent for the loss in caste which they had suffered in using the inhuman force of gravity during their descent. Darts which depended only on gravity were entirely disreputable.

In describing these experiments on darts I have tried to bring out the kind of problem which comes up in full-scale experiments when it is not possible to reproduce, during the tests, the exact conditions under which the article tested is to be used. The difficulties are frequently not very great and are of a type which a competent engineer can tackle.

**Model Experiments.**

I now pass on to cases in which it is not possible, or else merely undesirable, to perform full-scale experiments. In these cases we have to rely on experiments with models and on calculations which themselves are chiefly based on model experiments. The essential scientific question which arises whenever such experiments are carried out is that of how far they really represent, on a different scale, the action of the mechanism which is the subject of investigation. This question has to be discussed separately in each case, but there are a few general rules to which I may refer, and I will afterwards give an example to illustrate their application.

This is hardly the time or place to enter on a discussion of the principle of dynamical similarity on which the relationship between model and full-scale experiments rests. The principle itself is well understood; it is the application of it which leaves room for discussion and mistakes.

Suppose now that we know all about the motion of some dynamical system; and suppose we alter all the lengths in a certain ratio, all the velocities in another ratio and the masses in another ratio. We shall then find that we have altered all the other measurable quantities connected with the system in certain fixed ratios. If, for instance, we have doubled the lengths and kept the masses of corresponding parts unaltered, we will have decreased the densities of corresponding parts in the ratio 8 : 1, because we will have multiplied their volumes by 8 without altering their masses.

The principle of dynamical similarity amounts to a statement that the new system will be dynamically possible if all the forces are altered in a ratio which is equal to the product of the ratio in which the masses are altered multiplied by the ratio in which the accelerations are altered. It is, in fact, merely a direct application of Newton's law of motion, combined with the purely geometrical, or at any rate non-dynamical, principle that from any system one can derive a similar system by altering three, and only three, independent dimensions in any arbitrary ratios.

The difficulty in applying the principle is due to the fact that the forces concerned are due largely to causes which are not completely under our control. When we alter the dimensions of the system we alter the forces which arise in it owing to the weight, elasticity or other physical properties of the material

of which it is constructed. Unless these forces are altered in the correct ratio the new system will not be dynamically possible.

Unfortunately if, as is almost invariably the case, there are more than three distinct causes of forces in the system, it is impossible to find any system in which the forces due to all the causes are in the correct ratio.

It is therefore impossible ever to get complete dynamical similarity. On the other hand, it is frequently possible to guess, or it is known by experiment, that some of the causes produce very little effect on the motion. The art of using model experiments to deduce results, applicable when the dimensions of the system are altered, lies chiefly in selecting which of the possible causes of the forces in the system may safely be left out of consideration.

I do not propose to take up your time with a discussion of the ordinary application of the theory of dynamical similarity to experiments on the wind forces on models of parts of aeroplanes when placed in a wind channel. This has been discussed at great length both here and elsewhere. For the benefit of those who have not studied dynamical similarity, however, I may give a simple example of its application.

**Model and Full-Scale Yachts.**

Consider the relationship between the performance of a small sailing yacht and a model of it on a scale of one inch to one foot. The question is, could the performance of the model be used to find out anything about the performance of the full scale yacht? If so under what conditions?

In making our model experiment there are two necessary relations between the dimensions. The first is due to the fact that we are using the same liquid, water, in the two cases. For this reason the density of the model must be the same as the density of the full-scale yacht. The other condition is that the value of gravity, which is evidently a controlling factor, must be the same in both cases since both the model and the full-scale yacht sail on the earth's surface.

Now gravity has the dimensions of acceleration or rate of increase in speed. In order that accelerations may be the same in the two cases, when the linear dimensions of the system are altered, the speeds must be altered in proportion to the square root of the alteration in length. In the present case since the lengths are reduced from the full-scale to the model yacht in the ratio 12 : 1, the speeds must be reduced in the ratio $\sqrt{12} : 1$, or about $3\frac{1}{2}$ to 1.

If, owing to some other factor not yet considered, such as the viscosity or stickiness of the water, the model system is not similar to the full-scale system when the speeds of similar parts are in the ratio of the square roots of the lengths, then it will be impossible to use a model to represent the full-scale system. On the other hand it has been shown by experiments on the resistances of models of different sizes that when the speeds are in the correct ratio the systems are very nearly similar. Hence the use of a model is justified.

Now let us see how this works out in practice. Our full-scale yacht will be designed to sail her best with a moderate sea breeze of 25 miles per hour, and she then heels over through an angle of 10 to 15 degrees. Our model yacht 1/12th scale should therefore sail her best in a breeze of $1/\sqrt{12}$ times 25, or about seven miles per hour. Wind measurements made about a foot above the surface of an inland pond show that this is just about the speed of a "moderate breeze" there.

Models of about 1/12th scale are therefore good representations of full-scale yachts when they are sailed on an inland pond, because the wind-speed, close to the surface of the pond, happens to be on the average only $1/\sqrt{12}$ of the wind speed at sea. On the other hand you may have noticed that a model

yacht sailed at sea on a day when small boats are sailing well, will lie over almost flat on the water.

You will notice that when similarity is obtained the speed of the model should be $1/\sqrt{12}$, *i.e.*, about 2/7ths of the speed of the full-scale yacht. A yacht about 24 feet long will, when sailing her best, do seven miles per hour. A model yacht 24 inches long should therefore be capable of doing two miles per hour. Everyone who has watched boats on the Round Pond will recognise the truth of this statement. If one starts a good 24in. model boat across the Round Pond, one has just time to walk round to the opposite side before she gets there. A man walks about three miles per hour and his walk is about $1\frac{1}{2}$ times as long as the model's course. The model must therefore go at about two miles per hour. If therefore we take a successful model yacht about 24ins. long we may reasonably expect an exact replica 24ft. long to sail well in average weather at sea.

On the other hand, if we take a successful yacht 24ft. long, which sails well in a wind of 25 miles per hour, and make an exact replica four times the

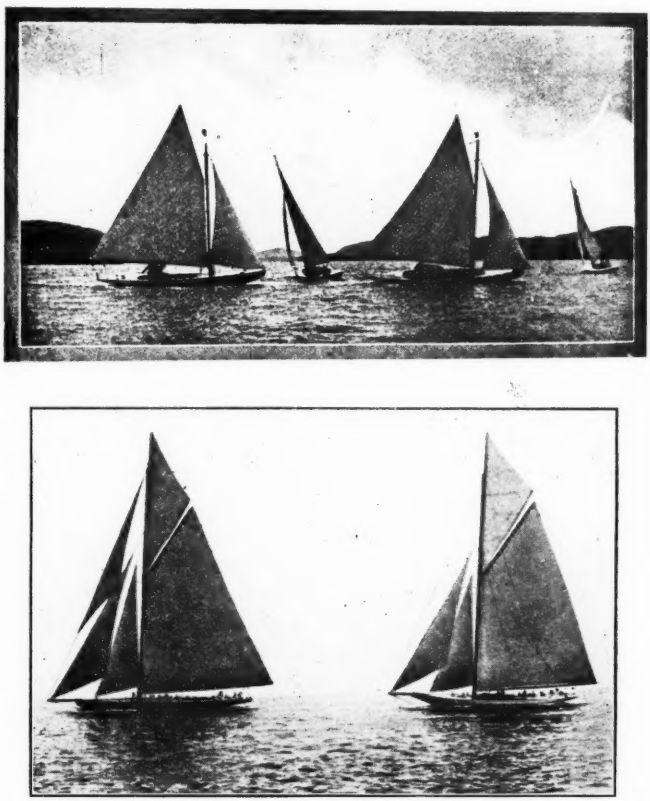

FIG. 2.—*Photographs showing comparative sizes of sails in large and small yachts. Above, 24-footers. Below, "Shamrock" and "Resolute" in race for the America's Cup. The photographs are reduced so that the lengths of the hulls appear the same in the two cases.*

size, that is 96ft. long, she will only sail her best in a gale of $\sqrt{4 \times 25}$, *i.e.*, 50 miles per hour. For this reason, therefore, large yachts need to have larger sails in proportion to their length than small ones.

I have here a slide (Fig. 2) showing (bottom photo) "Shamrock" and "Resolute" in the American Cup races last year, and (top photo) a race between yachts about 24ft. long. The scale of the smaller boats is increased till the two sizes appear the same length on the screen. You will see that the large yachts have much larger and higher sails in proportion. Even with these enormous sails the big yachts do not heel over to anything like the same extent as the small ones. I have here a slide (Fig. 3) showing "Shamrock" sailing on the day when her racing was abandoned owing to the high wind. You will see that she is hardly heeling over at all.

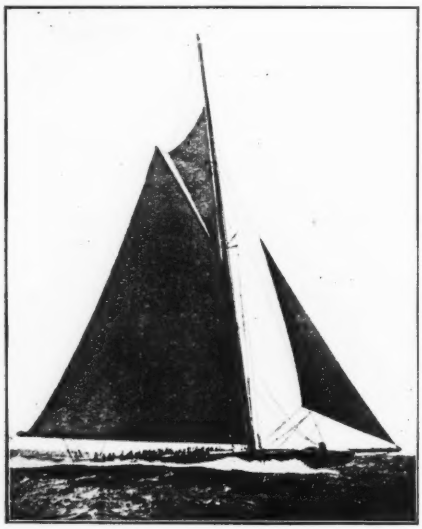

FIG. 3.—*"Shamrock" sailing on the day the race was abandoned.*

I have chosen the example afforded by model yachts because it seemed to me that observant people must recognise that the results arrived at by the argument of dynamical similarity are true in this case, and that when one recognises that the results are true it is much easier to understand the argument by which they are obtained.

**Example of Bomb and Pilot Bomb.**

We may now pass on to consider the way in which models may be used to solve more complicated problems in aeronautics. For this purpose I have selected an example from my own experience which has very little interest apart from the method of solution, and, as things turned out, no military value. Some time during 1915 various inventors got hold of the idea that the effectiveness of bombs was reduced by reason of the fact that they do not explode till they touch the ground. They thought that if a small pilot bomb could be made to travel in front of the explosive bomb, and if the two were connected by a wire which caused the upper bomb to explode as soon as the pilot bomb hit the ground, more damage would be done.

Accordingly they set themselves to designing elaborate electrical arrangements for exploding the upper bomb and complicated methods by which the pilot bomb could be projected from inside the main bomb during the descent. None of them seemed to notice the real difficulty of the problem, which is that of making the pilot bomb travel ahead of the main bomb when there is a connecting wire between them. At any rate those who mentioned it at all dismissed it with the inventor's general utility phrase, "Any competent engineer can do the rest."

One bomb made by an inventor was, I fancy, actually tried, and it was found that the pilot bomb came down well behind the main bomb.

When I was asked to examine the conditions under which a pilot bomb could be made to carry a wire ahead of the main bomb, the first idea which came up was naturally that if the upper bomb could be slowed up by some artificial means the scheme would certainly work. On the other hand the slower the bomb, the less accuracy would be expected in hitting the mark. It seemed likely, therefore, that a compromise would be necessary in which the bomb would be slowed up as little as possible consistent with the proper action of the pilot bomb.

Starting then with the idea that bombs should fall at least at 500ft. per second to ensure accuracy in aiming, I set myself to find out how much wire the best possible shape of pilot bomb could carry out in front of the main bomb at this speed. The first thing we found was that at 500ft. a second it was impossible to see how the pair fell. Accordingly I set myself to design a dynamically similar experiment in which everything happened more slowly. I found that if the main bomb were slowed down by attaching a small parachute to it so that it fell at 100 or 150ft. a second, it was quite possible to watch the action of the pilot bomb, especially if one stood just where it was expected to fall—it was not loaded of course.

Now let us see how the principle of dynamical similarity applies. Since gravity is evidently one of the most important factors, it will be seen at once from my previous remarks that to obtain a dynamically similar system in which the model bomb falls at 100ft. per second, that is 1/5th the speed of the full-scale bomb, it is necessary that the linear dimensions shall be reduced in the ratio 1 : 25. This would reduce the model to a very diminutive size, its weight being only 1/5,000th of the weight of the original system. Under these circumstances it would be impossible to see it, even when it was moving at only 100ft. per second.

It appears therefore that we cannot usefully perform an experiment in which similarity is completely preserved so that the accelerations and the densities are the same in the two cases.

On the other hand since the cause of the peculiar behaviour of the system appeared to be the force of the air acting sideways on the wire and driving the pilot bomb off its track, it seemed likely that the essential feature in the situation might be the ratio between the pull exerted by the pilot bomb and the resistance which the wire would experience if it were moved transversely, at the speed of descent. If this relationship were true then curves assumed by the string in all possible cases for which this ratio was given, would be similar.

This was found to be true. Here is a slide (Fig. 4) showing the results of experiments in which the bomb was dropped at 100ft. a second, with various lengths of connecting string.

As the length of string was increased the pilot bomb took up a position which was progressively farther and farther from the vertical line through the main bomb. A certain length of string brought the pilot to its lowest depth below the main bomb. The string then appeared to leave the main bomb horizontally.

As the string was lengthened still farther the pilot got dragged back by the wire till finally it was in a position behind the main bomb.*

In order to see whether the similarity relationship which I had guessed at was true, I took a certain easily recognisable shape for the connecting string, namely the one at which the pilot bomb was at its maximum depth below the main bomb. I then doubled its length and halved its diameter so that its resistance should remain approximately unchanged. I found that the string took up the same shape as before.

Next I tried increasing the speed to 150ft. per second and decreasing the thickness of the string in the ratio $(1.5)^2 : 1$, at the same time keeping the pull of the pilot bomb constant. Again the string took up the same shape.

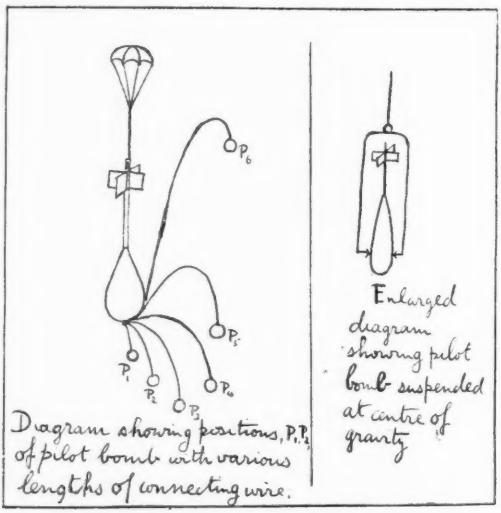
P6
P5
P1
P2
P3
P4
Diagram showing positions, P1 P2 P3 P4 P5 P6 of pilot bomb with various lengths of connecting wire.
Enlarged diagram showing pilot bomb suspended at centre of gravity

FIG. 4.—*Bomb and pilot bomb.*

It appears therefore that the predicted similarity relation is true.

The problem is therefore solved and one set of results with the model system gives a designer all he wants in designing any possible system consisting of a bomb and pilot bomb.

In describing these experiments I have tried to bring out the fact that the use of models does not end when it is impossible to get complete dynamical similarity, though the validity of the results arrived at in other cases must be verified experimentally by comparison of models on different scales. Nearly the whole of the model work now carried out is done under conditions in which there is no theoretical reason for supposing that dynamical similarity exists. On the other hand there is plenty of experimental evidence that model experiments, of the type done at the N.P.L. for instance, do very nearly agree among themselves even when the scales are very different; and that they do, in fact, very nearly represent the state of affairs in the full-scale.

**Aerodynamical Stability.**

Model experiments afford us a method of obtaining information which is

* When the pilot bomb got behind the main bomb it ceased to travel steadily, continually lagging behind and overtaking the main bomb.

useful in problems connected with aeronautical engineering. They do not as a rule help us directly in understanding the principles of aeronautics, for it is just as difficult to understand the model as it is to understand the full-scale mechanism.

The most important step which has yet been taken in understanding the principles of flight is that of Professor Bryan. Bryan treated the aeroplane as a rigid body acted on by forces which he specified in a form suitable for the application of dynamical equations. He was not concerned with the manner in which the forces arise owing to air pressure. He then investigated the oscillations and motion of an aeroplane in general terms.

Afterwards he made certain assumptions about the forces due to air pressure which were incorrect, and led to incorrect predictions about the behaviour of aeroplanes. The fact that his predictions were not all correct does not however detract, in the least, from the value of his work; for when the forces on parts of aeroplanes came to be measured by means of models, and when these results were substituted in Bryan's general equations in place of his original assumptions, it was found that the new results agreed very well with the observations of the behaviour of a full-scale aeroplane.

I need hardly take up your time with a discussion of the work of Bryan, Lanchester, Bairstow and Busk on aeroplane stability. I need only say that, as a result of their efforts, we have a pretty complete general understanding of the dynamics of flight, though, of course, we are no nearer understanding the more fundamental problem of how it is that the forces on a body moving in the air arise. Of this I shall have more to say later; in the meantime, an example of the application of Bryan's equations may be of interest.

Some time in 1915, it was required for some war purpose to carry a flat stream-line weight beneath an aeroplane at the end of a wire. In designing this apparatus we first made a flat weight and then put a fin behind it so that, when pivoted at the centre of gravity, the weight would head into the wind.

When we came to try this apparatus in the conditions under which it was to be used, we found that instead of hanging steadily the weight executed rapidly increasing oscillations, so that it quickly became quite dangerous.

We tried increasing the size of the fin, but that only seemed to add to its fury. At this stage it seemed that theory might be expected to help us by explaining the cause of the oscillations. I therefore modified Bryan's equations so as to make them applicable to a body hanging by a wire, and found that this rapidly increasing oscillation was exactly what the equations would lead one to expect. On the other hand I found that theory indicated that if, instead of increasing the fin behind, one were to cut it down till the weight would only just head into the wind when pivoted at its centre of gravity, then the increasing oscillations ought to disappear and the weight should travel smoothly at the end of its wire.

These conclusions were most strikingly verified. The weight was about 8ins. across. The fin was reduced by paring off successive small amounts from its after edge.

It was found that the weight remained unstable till its fin was only $\frac{1}{4}$in. deeper than the fin which was necessary in order to prevent the weight from lying broadside on to the wind. In this small range, involving only $\frac{1}{8}$in. of depth of fin, the weight travelled perfectly truly. I doubt if we should ever have found this small region of stability if we had not had Bryan's general equations to help us. A rough sketch of the weight and fins is shown in Fig. 5.

**Place of Pure Science in Aeronautics.**

So far I have dealt only with scientific methods which have a direct or at any rate an obvious indirect bearing on aeronautics, investigations in which the

scientist has in view, at any rate, some question relating to aeronautics. It seems to me however that these are not the kind of investigations which are likely

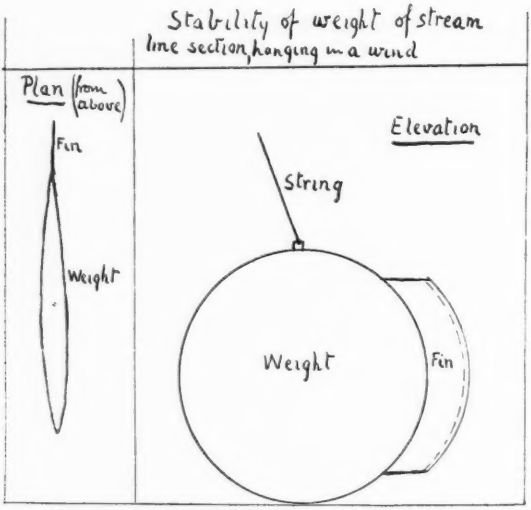
Stability of weight of stream line section, hanging in a wind
Plan (from above)
Fin
Weight
Elevation
String
Weight
Fin
Space between full and dotted edges of the fin shows the small range of sizes of fin for which the motion is stable.

FIG. 5.—*Stability of weight of streamline section hanging in a wind.*

to produce the greatest or the most profound changes in the science. Perhaps I can explain what I mean better by an imaginary case.

Try to imagine a race of men who lived entirely for golf, who thought about it at meals, practised it by day, dreamed about it at night and talked about it always. I do not suggest that such a race ever existed, but to the philosopher no conception is too revolting to be used in the practice of his art. This race would no doubt develop a class of highly specialised technical experts who would invent balls and clubs of the most complicated character, devise special apparatus for exercising the muscles used in doing a drive, invent special head-rests designed to fix one's eye on the ball, etc., etc. By means of this kind they would perhaps increase their drives from 200 to 300 yards.

At the same time there might exist a few lunatics who were so mad that they took not the slightest interest in golf, but preferred to spend their time in foolish inquiries about the action of food or drugs in promoting the selective growth of certain muscles, the factors which govern inheritance of physical characteristics, or other useless investigations. The chance that these lunatics would ever find out anything which had any bearing on golf would be extremely small; but it would always be on the cards that when they had really got a thorough understanding of the mechanism of the body, they might see how to breed a race of golfers who could drive 4, 5, 6 or 700 yards.

At any rate they, and not the technical experts, would be the only people who had any chance of making a really fundamental improvement in the length of a drive.

If our poor ignorant forefathers a hundred years ago had had the inestimable advantage of a Minister of Transport, do you suppose that he would have invented the twopenny tube? Of course not. His time would have been fully occupied in regulating the height at which a horse must carry his port and starboard lights. Perhaps the Wells of that time may have pointed out that the number of collisions is proportional to the square of the number of vehicles on the road, and that, at the rate of increase in population then current, movement on the surface of the earth would be impossible in 150 years or so. He might even have suggested that the transport problem would be solved by tubes underground in which the traffic went in one direction only.

None of these things would have brought electric railways the slightest bit nearer. The discoveries which made electric transport possible were not made by transport experts at all, but by Oersted and Michael Faraday, whose researches into the magnetic and mechanical effects of currents of electricity were instigated by their interest in pure science alone, and had no connection whatever with transport problems. They may even have been people who believed that rapid transport is on the whole a bad thing; that every increase in the facilities for travel merely decreases the size of the world instead of enlarging one's range; that the easier it is to get to China, say, the more European travellers would one find when one got there, and the less worth while would it be to go there. With these reactionary sentiments I should find myself in thorough agreement, but I should hardly expect to find sympathisers in this Society.

I may seem to be wandering from the point, but I introduce this matter to illustrate the idea that the far-reaching discoveries which revolutionise any branch of applied science are liable to be made by people who have no knowledge of or interest in the science in question, but who are chiefly interested in fundamental principles. What particular branch of applied science it is whose foundations they are examining, seems to me of very much less importance than the fact that they are examining foundations, rather than superstructure.

The difference between the methods by which a pure and applied scientist approaches his work is of some interest. If one compares the lives of distinguished applied scientists like Siemens, Marconi or Wilbur Wright with those of pure scientists like Kelvin or Lord Rayleigh one will at once be struck by the colossal difference in the range of investigations attacked by the two types of men.

The investigations of Lord Rayleigh extend over many branches of heat, light, sound, electricity, astronomy, hydrodynamics, aeronautics, molecular physics, mathematics and even psychical research. The investigations of Siemens are practically confined to one branch of electricity, those of Marconi to another and those of Wilbur Wright to one branch of aeronautics. This is not due altogether to lack of breadth of vision on the part of applied scientists. It is partly, no doubt, due to the fact that several applied sciences which are completely different in their aims rest on the same foundations, and a man who is dealing with those foundations, therefore, affects several applied sciences; but it is more I think due to the difference in the nature of the difficulties which are encountered in the two cases.

The applied scientist has in view a definite aim. He may not see the whole course of his work marked out before him, but he does at any rate see the direction in which he must go. The difficulties which he encounters are of a kind which perseverance, courage and ability will enable him to overcome. The labour is often very great, so great indeed that it absorbs all his attention, but on the whole he sees himself slowly and continually approaching his goal.

The pure scientist on the other hand finds difficulties in his path of such a formidable nature that it is often quite useless for him to attempt a straightforward approach to any particular distant goal. If he limits himself to a direct

attack he will in all probability find himself at the end of his life at exactly the same spot where he started as a young enthusiast. He is therefore forced to move in any direction in which it appears possible to proceed. The difficulties of his subject actually force him into other fields of science.

The realm of science may be compared with a mountain region in which the lower slopes are full of impossible precipices while the upper parts consist of long and difficult glaciers and snow slopes. The mountains are all connected together at their foundations, but the peaks are widely separated.

The applied scientist is like a mountaineer who forces his way to one of the summits across glaciers and through snow fields. The pure scientist is like the rock climber who, finding himself confronted with an impossible precipice, traverses out to one side on to another slope. Here perhaps he finds a short and difficult chimney which lands him at a higher level, from which he may, or may not, be able to traverse back to the peak on which he originally set his fancy. At any rate he is at a higher level than he was at the beginning.

In the course of his traverses prospecting for new routes upwards the climber will very likely see attractive possibilities about another peak, possibilities which could not be seen from a lower level. In that case he will, if he is wise, forsake his original scheme and try the new place.

**Examples from Author's Experiment.**

I have now tried to explain what I consider to be the main difference between research in pure and applied science. You will see from what I have said that it is to be expected that researches which were begun with a view to throwing light on the principles of aeronautics should, at times, have no obvious connection with aeronautics at all. With this by way of apology let us, shall I say, descend from the sublime to the ridiculous, while I mention a few of the bye-paths into which a search for fundamental principles in aerodynamics has led me.

The problem with which one is confronted is to find out how the air moves round a solid body which moves through it; and how the pressures which result from its motion arise. Practically no headway at all has been made towards the solution of this problem in any single case. The difficulty lies in the fact that at the surface of the body the air is churned up into a disturbed eddying state.

In the ordinary theory of hydrodynamics a fluid is contemplated which has no viscosity or stickiness; it slips without friction past the surface of the solid. The mechanics of such a fluid is amenable to mathematical treatment, and the result is arrived at that the air exerts no force on any body which moves uniformly through it.

The ordinary hydrodynamical theory is therefore quite inapplicable to the case of bodies moving steadily through the air. One must seek for the explanation of the forces which are observed in these cases in the action of the eddying region on the flow. The great difficulty is to represent that eddying region mathematically, and to explain how it arises. My first efforts were directed towards finding out everything I could about this eddying motion.

In the first place, though I could find no way of representing, mathematically, the actual motion of each particle of fluid, I succeeded in finding a way of expressing the effect of the whole turbulent region on the average condition of the fluid in certain cases. In the case of the turbulent region which occurs when the wind blows over the surface of the earth, I found relationships between various things which depend on the existence of the turbulent motion. I connected the rate at which the wind varies above the surface of the earth, the daily variations in the temperature at the top of the Eiffel Tower and the thickness of the fogs on the Grand Banks of Newfoundland, which I had previously

measured during the summer of 1913. I calculated also the amount of friction which the wind exerts on the ground, and found that it is the same as that measured in a wind tunnel, using a smooth flat plate placed edgewise in the wind.

These results seeming promising, I next tried to find out something about the way in which energy is dissipated in eddying motions. I found at the outset from mathematical considerations that in order that energy may be dissipated in eddying motion, this motion cannot be confined to two dimensions. It must extend to three. On the other hand if the wind is blowing over a large expanse of flat ground one would expect, *à priori,* that the friction on the ground might produce eddies of wind like rollers which would have the effect of making the variations in wind occur in the direction of the wind and vertically, but not across the direction of the wind.

To investigate this question I had a light wind vane made* which automatically recorded the horizontal and the vertical direction of the wind at any time. This instrument showed that the wind in eddying motion varies in a cross-wind direction through exactly the same amount on the average, that it varies in a vertical direction.

Further evidence showed that the variations of wind along the direction of the wind are exactly the same as in the other two directions. That, in fact, there is in the case of a natural wind an equipartition of eddy energy in all directions in space.

The method adopted for calculating the energy dissipated in eddying motion seemed to be also available for calculating the energy dissipated in tides. My calculations showed that a very much greater amount of energy is dissipated by the tides than had previously been supposed. This result appeared important because it showed that water tides may be the cause of the gradual slowing down of the earth which astronomers have been able to observe. The question has lately been taken up by Dr. Harold Jeffreys, who applied my method to all existing basins where tidal energy might be dissipated, and showed that the tides do in fact dissipate just about enough energy to account for the observed slowing down of the earth.

Already you see how investigations which were begun in relation to aerodynamics have invaded meteorology, tidal theory and astronomy. I need hardly follow their further ramifications into investigations of the oscillations of tides in gulfs and rectangular basins, the height of tide in the Bristol Channel, the dissipation of sound in the atmosphere, the cooling of thermometer bulbs wetted with organic liquids, the production of fog, etc.

Not long ago I returned to the question of the motion of bodies in fluids, and tried to attack it from a new aspect. Instead of trying to find out about the turbulent motion at the surface of the body, I began to search for cases in which the flow does not depend on the turbulent region. I found a rich field in the dynamics of rotating fluids. I cannot here go into the arguments which induced me to believe that I ought to be able to predict certain types of result about the dynamics of rotating fluids; and that, contrary to all experience with non-rotating fluids, these mathematical results were likely to be true experimentally when tried with real fluids. I see no way in which the arguments can be explained in non-mathematical terms. I must ask you to take them on trust therefore, and to look at a few slides showing how completely mathematical theory is verified.

First of all I predicted that if a solid cylinder of the same density as water is towed through a rotating liquid, say from side to side of a round, rotating

* This was kindly undertaken for me by Mr. W. H. Dine, F.R.S.

trough, it would move straight through just as if the liquid were not rotating and would pass through the centre of the trough.

A sphere, however, under the same conditions should be deflected and should pass to one side of the centre.*

The next prediction was that if any small or slow motion be communicated to a rotating liquid it must take place in two dimensions only. A consequence of this is that if you start a small motion in a rotating liquid and then put in a drop or two of coloured liquid to mark it as it moves, the coloured liquid will be drawn out into sheets which are everywhere parallel to the axis of rotation. These sheets when seen by anyone looking along the axis of rotation should appear as lines, and if the liquid does move in the way predicted by theory, a photograph taken by a camera on the axis of rotation will show the colouring matter spreading out as a thin line, instead of the usual irregular diffusion which happens in a non-rotating liquid. In Fig. 6 such a photograph is shown.

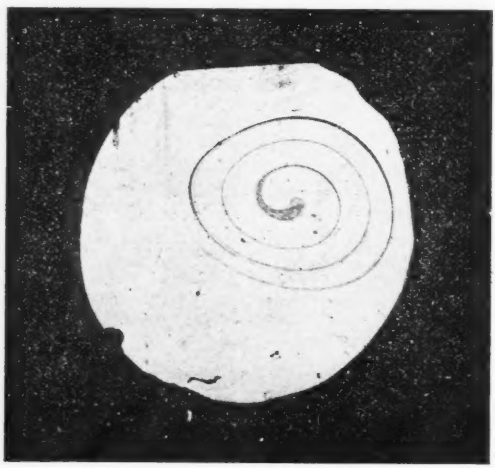

FIG. 6.—*Photograph of thread of coloured liquid in a rotating liquid, showing extreme thinness of the sheets into which the coloured liquid is drawn.*

It will be seen that this prediction also is completely verified.

The fact that the prediction that small motions in a rotating fluid are two-dimensional was so completely verified naturally led me into the inquiry: If the boundaries of the fluid move in such a way that the motion of the liquid cannot be two-dimensional, what happens? The answer must be that in this case the motion cannot be small everywhere.

I don't want to take up your time in pursuing the ramifications of this subject, but one result which came out has perhaps an important bearing on the foundations of aerodynamics. I solved the equations to the motion of a sphere moving uniformly along the axis of a rotating fluid. The equations which I used were the ordinary hydrodynamical ones which allow the fluid to slip over the surface of the sphere. One would expect therefore *à priori* that the results

* Two slides were shown illustrating these points. They are not here reproduced.

would be quite as useless as they are in the case of a non-rotating fluid. However, when I came to work out from my equations how much slip the ordinary theory leads one to expect at the surface, I found the extraordinary result that in this case there is no slip at all provided that the sphere itself is not rotating. That is to say the sphere should carry with it a sort of envelope or sheath of liquid which is not rotating or slipping past it.

In this special case, therefore, you will see that the circumstance which prevents the ordinary equations from representing the motion has disappeared, and there is a reasonable chance that they will, for the first time on record I believe, really represent the motion of liquid when a solid body moves in it.

It is difficult to devise any method for showing up the motion of a liquid in this case, but there is one prediction from theory which it should be easy to verify. If the non-rotating sheath predicted by theory really exists, then the sphere should move without rotation. If the sphere, originally rotating with the liquid and at rest relatively to it, begins to move along the axis of rotation, it should stop rotating, and should only start rotating again when it stops moving along the axis. This prediction also is verified. (The apparatus was shown and the experiment successfully performed.)

**Conditions Favourable for Research.**

I have tried this evening to bring into prominence the differences between the methods used in pure and applied science. It would be a pity to leave the subject without some reference to the conditions under which the various types of research work can most favourably be carried on, or suggesting how these conditions affect the possibilities of organising research.

Perhaps the most striking feature of aeronautical research at the present time is the large development of the use of models which has taken place during the last few years. Almost every change in design is now tested first in a wind tunnel.

With the rapidly increasing number of tests with which our wind tunnels have to cope, their staffs have increased and their methods have become standardised to such an extent that they are now large departments employing perhaps 50 or 60 people. The chiefs of these departments are good scientific men, but the success of the larger part of their work, which is routine work, will depend more on their organising ability than on their scientific attainments.

When we visit the National Physical Laboratory, the Royal Aircraft Establishment or the American National Aeronautical Laboratories we find magnificently equipped laboratories and highly trained staffs who are full of enthusiasm for scientific research. Their work is admirably organised, so that the wind channels are very seldom out of use, and every member of the staff has his time fully occupied from 9 to 5, or whatever their hours of business happen to be. It is difficult to see how more tests could possibly be carried out with the means at their disposal, or how they could be done better.

On the other hand it is by no means certain to my mind that the kind of organisation which is most suitable for routine test work is also the most suitable for scientific research.

To the unscientific visitor, an administrative officer or cabinet minister for instance, there is no difference between them. In both cases use is made of the same material, wind channels and instruments, and both are done largely by the same young men. There is, however, a vast difference between the calls which the two types of work make on the minds of the experimenters.

Tests, it seems to me, consist of a series of operations which a properly trained man can do at any time he is called upon, provided he is in sound physical health. They are usually intended to give numerical information about some

particular feature in design, and the form in which the answer will appear is known beforehand. In many cases the report on the test could almost be printed off beforehand, blank spaces being left for filling in figures. The time taken to do a test can be estimated by an experienced man as so many man-hours. Any tester who happens to be vacant at the time can be put on to a job, and the organisation of an establishment devoted to tests presents much the same problem to its chief as that of making up a railway timetable, or organising a machine shop.

Scientific research, on the other hand, is a very different matter. One is seeking for a principle, an explanation, or perhaps only the best shape for a part of a design, but the essential characteristic is that one does not know beforehand the form in which the answer is going to appear. One cannot estimate beforehand how long it will take. It occupies one not only from 9 to 5, but also from 5 to 9. I don't mean, of course, that one must work at it for the whole 24 hours, but that one's mind will keep going back to it at all times of day and night. One may work for weeks at it without a vestige of result, and then the result may come to one in the middle of the night, or during a chance conversation with a friend. It is even more likely that one may work for weeks or even months at a subject, never get any way with it at all and finally give it up with absolutely nothing to show for one's labours. Again, one man may do as much in a week as another in a month and may tire himself more during his more concentrated effort.

For these and other reasons, it seems to me that it is a problem of almost insuperable difficulty to organise an establishment for research on the man-hour principle. As to how it can be organised, I have no suggestions to offer. I can only say that in my opinion research work is so difficult and exacting that a man can only turn out his best work if he is completely free to go where his researches lead him, free to choose his own time for work, free from other duties which would divide his mind, and lastly, free to sit down and produce no visible result for weeks on end.

Under these conditions the applied scientist should, if he is a good man, make steady progress towards his goal. On the other hand, for the reasons I have already discussed, the pure scientist might make no progress at all in the direction towards which he set out. If he is a good man he will make progress in some direction or other, but that direction may not be one in which the particular institution which employs him has any interest.

The man who is doing research in applied science presents a difficult problem for the organiser, but the pure scientist is still more difficult to fit in, unless the institution which employs him is prepared to accept work covering a very large field indeed.

For this reason it seems that an institution which exists in order to further any particular applied science cannot employ a pure scientist so efficiently as an institution like a University or like the Department of Scientific and Industrial Research, whose interests cover the whole range of science.

**Conclusion.**

I expect that some members of the audience will be disappointed in a lecture about aeronautics which tells them nothing about aeroplanes. To the scientist an aeroplane is merely a complex body moving through a fluid, and until he understands how a simple body moves he has no chance of understanding the fundamental principles of aeronautics. I have chosen my examples of scientific methods in aeronautics for their merits as good illustrations of these methods, and for their novelty rather than for their usefulness in the practice of aviation.

If I have succeeded in producing in the minds of people engaged in the

administrative and practical side of aeronautics some idea of the difficulties encountered by the pure scientist in his search for fundamental principles, some idea of the way in which these difficulties react on his work, broadening his outlook and forcing him into several fields of science, and finally, if I have conveyed any idea of how important freedom is to him in order that he may carry out his best work, I shall feel that I have not spoken in vain.

# THE PROBLEM OF FLAPPING FLIGHT.

BY HERBERT CHATLEY, D.SC. (ENGRG.) LOND., M.INST.C.E.I., ASSOC.M.INST.C.E., ASSOCIATE FELLOW.

It is only within the last few decades that it has been possible to conceive of a flying machine operating on the flapping system which would be consistent with mechanical principles. The development of gliding or "wedge" flight through the medium of the screw-propelled aeroplane has largely contributed to this result, but it must not be overlooked that as far back as 1899 Prof. Maurice Fitzgerald (Trans. Roy. Soc., Vol. LXIV., p. 420, and also (A) Vol. LXXXIII., p. 72, 1909) and Rayleigh (Manchester Lit. and Phil. Soc., Vol. XLIV., Pt. 2, No. 5, pp. 1-26—see Appendix III. hereto) enunciated certain of the most important factors of the problem. Whether the mechanical action should be the same as that of a bird is a matter for discussion, but anyway it is certain that a moving surface having a periodic vertical motion and a periodic change of "attitude" can efficiently produce lift and propulsion. The success of several rotary feathering lifting wheels (*e.g.*, Clarkson's and Pichou's, concerning the latter see "L'Aerophile," March 1st, 1912) in producing appreciable lifts per horse-power is significant, but it is probable that the inventors have not sufficiently realised:—

(1) The changes in relative motion which will occur during horizontal translation, and

(2) The increase of lift available during the acceleration of a flapping wing.

In a short article (partially based on Fitzgerald's and Rayleigh's papers, supplemented by a consideration of the effects of "attitude") contributed by the author to the Junior Institution of Engineers ("Journal," April, 1909, reprinted in "Fly" (U.S.A.), March, 1912, and appended hereto as Appendix I.), the first question is to some extent dealt with. The second forms the subject of Fitzgerald's later paper (1909).

Further complications will arise from the vertical motions of the body of a machine due to the periodic variation of the lift above and below its mean value (which is equal and opposite to the total weight), but it is probable that the vertical velocities so developed will be small under well arranged conditions.

---

Let it be supposed that a flapping wing machine possesses a forward horizontal velocity with respect to the air of 60 feet per second and that it is sustained by a flapping wing having a vertical range of 4 feet and a periodic time of one-fifth of a second. Further, let it be supposed that the displacement of the wing with respect to its mean position (on the body) varies harmonically. Then the vertical velocity of the wing with respect to the body is $V \cos \theta$, where

$$\theta = 2\pi t/T,$$

$t$ being the period since the beginning of the stroke cycle and $T$ the periodic time. The mean vertical velocity is 40 feet per second and the maximum velocity is 62.8ft. per second.

Neglecting the oscillations of the body, the relative velocity of the wing to the air at any instant is

$$\sqrt{\{60^2 + (62.8 \cos [10\pi t])^2\}}$$

and the direction thereof is inclined to the path at an angle

$$\tan^{-1}\frac{62.8\cos[10\pi t]}{60}$$

(Note that this has a maximum value of about 45°, which is, according to Pettigrew, the angle of maximum wing twist in flying animals.—Proc. Roy. Soc., 1866.)

If, during the downstroke, the angle of attack (chord of wing to relative air motion) be constant (say 5 degrees) and during the upstroke the said angle is zero, it will be found that an appreciable lift and propulsive force are developed. The calculation is best made by finite differences or graphical interpolation and planimetry.

Instead of a constant angle of attack, alternating with zero, a periodic change of " attitude " from six degrees to zero will give good results, as the table below will show.

In this table it is assumed that

(1) The reaction varies as the square of the relative velocity and as the angle of attack *plus two degrees.*

(2) The resultant acts one and a half degrees *ahead* of the normal to the chord.

This indicates that the lift during the half cycle of descent of the wing from the mid-position and the reascent to the mid-position varies from 45/24 to $10\frac{3}{4}/24$ of the mean value and that the drift is propulsive and can overcome a mean resistance equal to one quarter of the mean value of the lift, *i.e.*, one quarter of the weight supportable as a similar result is obtained in the other half of the cycle.

It will, of course, be understood that the analysis given is only approximate and intended for illustration. The researches of Eiffel, Riabouchinski, and others indicate the reactions in a steady stream to be of the order given, but it is somewhat doubtful what effect the accelerations and rotations will have. The relative velocity of the wing to the air changes periodically and it is uncertain whether the conformation of the streamlines will be the same at any instant of the cycle as that which would occur with a steady velocity identical with the instantaneous velocity. The presence of a " virtual mass " of fluid during acceleration necessarily implies some modification so that while the reaction will be quantitatively changed by the addition or subtraction of the product of the virtual mass into the acceleration, the actual aerodynamic effect may also differ. Bryan's rotative coefficients come into the question and when comparing with an aerofoil moving in a steady stream it should be remembered that the root mean square of the velocity is more important than the mean.

Further, it must be remembered that the acceleration alternately upwards and downwards of the whole mass will affect the problem. It is doubtful whether the vertical velocities of the body will attain values having any appreciable effect on the relative velocity, but the angle of attack being small may be so modified to an extent which will alter the reactions. If, for example, the sustaining force during the upstroke were zero, during rather more than half a cycle the body will be accelerating downwards under the force of gravity. Supposing, roughly, that the downward velocity during a half cycle alone needs to be considered, then its maximum value is $\frac{1}{2}gT$ where $T$ is the periodic time. (Thus, if $T$ is 0.2 seconds, the maximum downward velocity is 3.2 feet per second. If the forward velocity is 60 feet per second, this makes a difference of approximately 0.05 radians or say three degrees in the attitude of the wing.)

TABLE SHOWING LIFT AND DRIFT ON AN OSCILLATING WING; RANGE 4 FEET; PERIODIC TIME $\frac{1}{5}$ SECOND; HORIZONTAL MOTION 60 FEET PER SECOND; ANGLE OF ATTACK VARIES FROM 6° TO 0°.

| Epoch in Cycle. | V. cos $\theta$ | $\tan^{-1}\left(\frac{V.\cos\theta}{60}\right)$ | Angle of Attack $\gamma$ | Wing Angle. | Direction of Resultant. | Lift Coefficient. | Drift Coefficient. | $60^2 + V^2 \cos 2\theta$ | $\gamma + 2°$ | Product. | Arbitrary Units. Lift. | Mean Lift. | Drift. | Mean Drift. | Remarks. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 0 | — 62.8 | — 46° 10′ | + 6 | — 40° 10′ | 48° 20′ | 0.7470 | 0.6656 | 7544 | 8° | 60352 | 45 | | 40 | | Midpoint. |
| | | | | | | | | | | | | 42 | | 35 | |
| T/12 | — 54.4 | — 42° 10′ | + 5½ | — 36° 40′ | 51° 50′ | 0.7863 | 0.6189 | 6559 | 7½ | 49192 | 39 | | 30 | | |
| | | | | | | | | | | | | 33 | | 26 | Wing descending. |
| T/6 | — 31.4 | — 27° 40′ | + 4½ | — 23° 10′ | 65° 20′ | 0.9087 | 0.4184 | 4586 | 6½ | 29802 | 27 | | 12 | | |
| | | | | | | | | | | | | 22½ | | 5¾ | |
| T/4 | + 0 | 0 | + 3 | + 3 | 91° 30′ | 0.9997 | —0.0262 | 3600 | 5 | 18000 | 18 | | — ½ | | Bottom of Stroke. |
| | | | | | | | | | | | | 18 | | — 5½ | |
| T/3 | + 31.4 | + 27° 40′ | + 2½ | + 30° 10′ | 118° 40′ | 0.8774 | —0.4807 | 4586 | 4½ | 20667 | 18 | | —10 | | |
| | | | | | | | | | | | | 17½ | | —12¼ | Wing ascending. |
| 5T/12 | + 54.4 | + 42° 10′ | + 1½ | + 43° 40′ | 132° 10′ | 0.7412 | —0.6721 | 6559 | 3½ | 22956 | 17¾ | | —14½ | | |
| | | | | | | | | | | | | 14 | | —12½ | |
| T/2 | + 62.8 | + 46° 10′ | 0 | + 46° 10′ | 134° 40′ | 0.7112 | —0.7037 | 7544 | 2 | 15088 | 10¾ | | —10½ | | Midpoint. |
| | | | | | | | | | | | Average | 24 | | 6 | |

NOTE.—The second half of the cycle, wing ascending from the midpoint and descending to it again, is the same as the above but in *reverse* order.

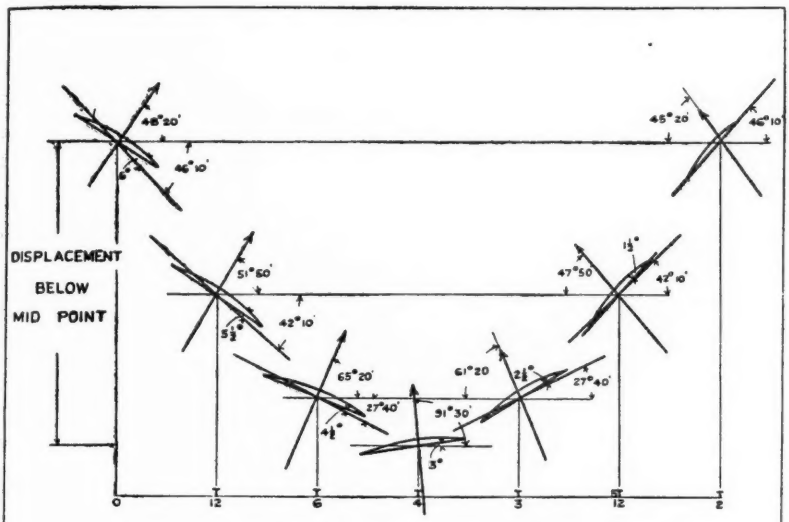


Fig 1. VARIATION OF DIRECTION OF REACTION DURING HALF CYCLE.

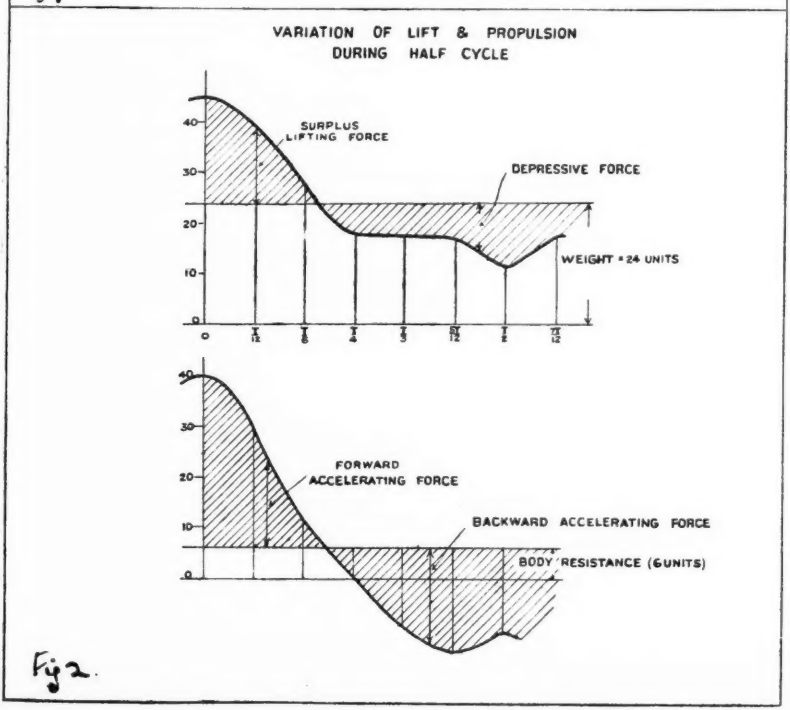


Fig 2.

The mean resultant of the forward thrust, as computed in the manner given above, has to overcome the body resistance only and as for good gliding conditions the body and wing resistance should be equal and the total resistance not more than one sixth the weight, it is not absolutely necessary that the said resultant should have a mean value more than one twelfth the weight. In any case, however, there is a periodicity in the value of the thrust with *negative* (backward) values during the upstroke (unless lift is sacrificed) so that the body will be subject to an alternate acceleration and retardation. In view of the fact that the mean thrust is but a fraction of the weight and the range of variation is not very great, the changes of velocity so produced will be less than the vertical changes so that when the velocity of translation is appreciable the effects of this horizontal variation can only be very small.

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A careful study of the problem on the above lines clearly indicates the possibility of constructing an ornithopter. The mechanical difficulties remain to be considered. Two wing motions are necessary—

(1) A vertical linear reciprocation.
(2) An angular reciprocation.

These two need to be in "phase" and moreover must occur at the prescribed rates. The attainment of the latter result is not very easy. In view of the greater resistances during the downstroke there will be a tendency for this to occupy more than half the periodic time, the upstroke being correspondingly reduced. While this is of itself an apparent advantage it must be remembered that such a change from the hypothetical conditions involves alterations in the direction of relative motion. For this reason it would seem necessary that the vertical reciprocation should be produced by a "slider-crank" transmission from a rotating shaft with a flywheel or some other energy reserve to maintain regularity during the cycle. The torsional oscillation could be produced by a rigid transverse shaft through the wing rotated by gear wheels actuated by the vertical slider or by means of warping wires running over pulleys mechanically connected to such gear wheels. Whether the wings should rock on a fulcrum attached to the body (as with birds where it is a physiological necessity) or rise and fall in toto must be left to the designer to decide (see Appendix II.).

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Three important questions must next be discussed:—

(1) Starting.
(2) Alighting.
(3) Stability.

**Starting.**

If the velocity of translation be omitted in the calculations indicated above, the angles of attack become very large, and without wishing to make any final pronouncement on the subject, the author should imagine the appliance would accelerate upwards and forwards until the conditions of equilibrium prescribed are attained.

An elevator such as is employed in aeroplanes would probably serve to determine the level at which horizontal flight should commence. Ascent, of course, implies extra power, but since the velocity of translation during ascent is less, a comparatively small reserve of power should prove sufficient.

**Alighting.**

If the mechanism is stopped and the wings locked in a proper position, descent by volplane will be possible. A few flaps just before contact with the ground should damp out the vertical velocity so that the impact may be small.

**Stability.**

The structure of a bird seems to indicate that the general principles of stability in the case of an aeroplane apply also to the ornithopter. There are, however, two further possibilities to consider:—

(1) Cumulative disturbance due to synchronism of the reciprocation with the natural oscillation of the appliance considered as a glider.

(2) Similar synchronism of the reciprocation period with that of gusts.

The necessity of avoiding such synchronisms clearly leads to the adoption of a very short period for the reciprocations. Few birds make less than five beats per second and probably this is about the minimum number desirable. Gusts rarely have a periodic time of less than one second and more generally take from five to ten seconds to complete a cycle. As to the natural oscillations, it will be necessary to compute these in accordance with the Bryan theory and take precautions to assure a difference between these and the reciprocation period.

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The above notes were written before the war, but nothing has since happened seriously to invalidate the figures. The mechanical argument is apparently unassailable. The difficulty lies in the mechanism required.

## APPENDIX I.

From "Fly Magazine."

### APPLICATION OF THEORY TO ORNITHOPTERS. THE ACTION OF THE FLAPPING WING.

BY HERBERT CHATLEY, B.SC. (ENG.), A.M.I.C.E.I.,

PROFESSOR OF CIVIL ENGINEERING, TANG SHAN IMPERIAL ENGINEERING COLLEGE.

The essential feature of the ornithopter is the reciprocating motion of the supporting surfaces. A study of bird flight shows that in nearly all cases ascending flight is performed by reciprocating motion, horizontal flight by reciprocating motion in most cases, and descending flight almost always by gliding. The wings therefore act to some extent in the same manner as the "aerofoils" of a gliding machine, and it may be true (as most aeroplanists assert) that with a continuous means of propulsion (the propeller) gliding flight is mechanically preferable in all cases. Nevertheless there is the great advantage in bird flight that no great initial velocity is required, and there is no loss of energy due to an indirect means of propulsion. Hence there still exists a large class of would-be aviators who believe in the flapping type.

The researches of Borelli, Pettigrew, Marey and Mouillard have supplied much information as to bird flight, but no exact knowledge was obtained until Marey made his cinematographic and sphygmographic observations. It then appeared that the wing twisted during reciprocation (see diagrams in my book, "The Problem of Flight," Chap. III.), and it immediately became conceivable that the wing acts as an aerofoil does, and not with a simple beating action. It is difficult to see, if the wing does not twist, how the difficulty of the down thrust on the upstroke can be prevented.

Lord Rayleigh in his famous paper to the Manchester Literary and Philosophical Society, in 1900, gave a tentative mathematical analysis of bird flight, but does not seem to have definitely considered the "attitude" of the wing in the different points of its path, but the following investigation is on very similar lines to those suggested by him. As a matter of fact, I think that the aforesaid

paper shows a more general knowledge of the fundamental principles of flight than any work which has appeared until we reach Lanchester's book. Many of Lanchester's important conclusions as to friction and economics of flight are derivable therefrom, and most of us have been blind these few years.

The most important point in connection with the action of the wing is the direction of its motion relatively to the air. *This is not the same as that of the body*, but differs therefrom by a certain angle. If the vertical velocity of reciprocation of one point of the wing at a certain time is $v$, and the body has a horizontal velocity $V$, then the wing is proceeding in an inclined direction upwards or downwards (according as $v$ is upwards or downwards), the inclination being

$$\theta = \tan^{-1}\frac{v}{V} \quad . \quad . \quad . \quad . \quad . \quad (1)$$

with the horizontal.

In order to simplify the mathematical treatment of the subject I will make the following preliminary assumptions:—

(1) That the wing is an aeroplane. This is quite untrue in the case of birds, but we can modify it to suit aerocurved surfaces afterwards.

(2) That the reciprocation is wholly vertical. This is also untrue, since the wing swings about its root in an arc, but, as the horizontal component of the velocity at each position in symmetric flight will neutralise that on the other wing, it is only necessary to find the point on the wing at which there is the mean effective velocity, in order to convert from that velocity to angular velocity of the wing about its axis in the root.

(3) That this vertical velocity of reciprocation varies harmonically throughout the cycle, and that the "attitude" of the wing similarly varies, but not necessarily in the same phase.

(4) That the line of flight is horizontal.

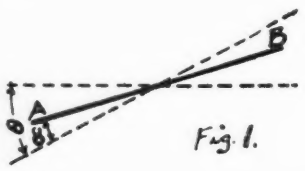


Fig. 1.

The diagram Fig. 1 represents the aeroplane $A\ B$ in section, with a forward horizontal velocity $V$ and a downward velocity $v$. The actual velocity in still air will then be

$$U = \sqrt{(v^2 + V^2)} \quad . \quad . \quad . \quad . \quad . \quad (2)$$

The angle of attack (*i.e.*, the angle between the aeroplane and the direction of relative motion) is $\alpha$, and the reaction on the surface is then

$$N = KAU^2 \sin\alpha \quad . \quad . \quad . \quad . \quad . \quad (3)$$

There will be a vertical and upward component

$$L = N\cos(\theta - \alpha) \quad . \quad . \quad . \quad . \quad . \quad (4)$$

and a horizontal and forward component

$$T = N\sin(\theta - \alpha) \quad . \quad . \quad . \quad . \quad . \quad (5)$$

In order to express the position of the plane with reference to the axis of the body (in this case horizontal) I will call $(\theta - \alpha) = \beta$, so that $\alpha = \theta - \beta$.

It should be observed that $T$ here acts as a propelling force, and is opposed by a frictional resistance depending on the form and area of the wings and body, and to a very slight extent, on the position of the wing. We will call this resistance $R$. Under conditions of turbulent motion

$$R = \mu V^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

The vertical component of this resistance will be negligible.

Hence the vertical equation of motion is

$$M\frac{d^2z}{dt^2} = Mg - KAU^2 \sin(\theta - \beta)\cos\beta \quad . \quad . \quad . \quad (7)$$

and the horizontal equation of motion is

$$M\frac{d^2x}{dt^2} = \mu U^2 - KAU^2 \sin(\theta - \beta)\sin\beta \quad . \quad . \quad . \quad (8)$$

These are not integrable equations because $U$ and $\beta$ are unknown functions of the time.

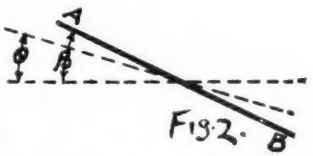


Fig. 2.

Fig. 2 represents the wing ascending and the notation employed above can be again used. *In this case, however, the reaction cannot produce a forward component.* For $\beta$ being greater than $\theta$ there is a lift, but the reaction is directed backwards, and the horizontal component will cause considerable retardation.

If $\theta$ is greater than $\beta$ then there is a reaction on the upper surface and the wing is subject to a down thrust. Even if this were permissible to a slight extent the forward component would not be appreciable without a very considerable down thrust.

Hence we see that values of $\beta$ either greater than or less than $\theta$ are aerodynamically objectionable, and we have the conclusion that $\beta = \theta$ during the upstroke.

This condition will not necessarily apply when both $\theta$ and $\beta$ are very small, since then the resistance due to an excess of $\beta$ over $\theta$ would not be very appreciable, and the prolongation of the lift would be useful. Furthermore, in the case of aerocurves the reaction is directed somewhat a little forward of the normal, so that with such aerocurves a small excess of $\beta$ over $\theta$ when both are small would not necessarily cause any aerodynamic resistance.

Reverting now to the downstroke we see that in order that this should be most effective the ratio of the thrust to the lift should be maximum. This depends on both $\beta$ and $\theta$, and is identical with the ordinary problem of gliding. There are two criteria for the value of $\theta - \beta$ (the angle of attack):—

(1) It should be about 6° for an aeroplane (Lanchester, Ferber).
(2) It should be about half the gliding angle $\theta$ (Penaud, Ferber).

Employing the first condition we see that when the velocity of reciprocation is a maximum (*i.e.*, $\theta$ is a maximum)

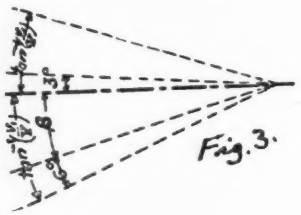
$$\theta - \beta = 6° \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

Given the maximum velocity of reciprocation $v_1$ we have then a means of finding the two maximum values of $\beta$. On the upstroke it has been shown that $\beta = \theta$. Hence we have

$$\beta_1 = \tan^{-1}\frac{v_1}{V} \quad . \quad . \quad . \quad . \quad . \quad (10)$$

and on the down stroke

$$\beta_2 = \tan^{-1}\frac{v_1}{V} - 6^\circ \quad . \quad . \quad . \quad . \quad (11)$$




These conditions can be satisfied if we suppose that $\beta$ varies harmonically in the same phase as $v$ from a value of 3° to a range of

$$\tan^{-1}\left(\frac{v^1}{V}\right) - 3^\circ$$

above and below this (see Fig. 3).

Hence expressing $v$ and $\beta$ as functions of the periodic time $\tau$ we have

$$v = v_1 \sin\left(\frac{2\pi}{\tau}t\right) \quad . \quad . \quad . \quad . \quad . \quad (12)$$

and

$$\beta = 3^\circ + \sin\left(\frac{2\pi}{\tau}t\right)\left\{\tan^{-1}\left(\frac{v^1}{V}\right) - 3^\circ\right\} \quad . \quad . \quad (13)$$

so that we have the following table of values:—

| | Epoch. $\frac{2\pi}{\tau}t$ | Velocity. $v$ | Attitude. $\beta$ |
|---|---|---|---|
| Beginning of upstroke or end of downstroke ... | $0\ (\tau = 0)$ | 0 | 3° |
| Middle of upstroke ... | $\frac{\pi}{2}\ (\tau = \frac{1}{4})$ | $v^1$ | $\tan^{-1}\left(\frac{v^1}{V}\right)$ |
| End of stroke ... ... | $\pi\ (\tau = \frac{1}{2})$ | 0 | 3° |
| Middle of downstroke ... | $\frac{3\pi}{2}\ (\tau = \frac{3}{4})$ | $-v_1$ | $-\tan^{-1}\left(\frac{v^1}{V}\right) + 6^\circ$ |

Marey's diagrams may be tested by comparing the values of $\beta$ given in formulæ 10 and 11, with his diagrams of the wing cycle ("Le Vol des Oiseaux," "Animal Mechanism," and my own book, vide Suprâ). The agreement is fairly close.

It should be observed that the quantity $v_1$ is involved. As far as I recollect the maximum values of $\beta$ are about 25°, so that $\tan^{-1}(v_1/V)$ equals about 20°, and $v$ therefore equals 0.36 $V$. This will be the velocity of a point about $\frac{2}{3}$ the length of the wing from the root.

In correcting for pterygoid section it is necessary to note that the reaction is inclined about 2° or 3° forward from the normal to the chord of the curvature, so that the resistance is decreased. This correction will more particularly apply to the resistance question considered below.

**On the Lift and Horizontal Force in Relation to Weight and Head Resistance.**

I have given above formulæ relating to the lift and horizontal component of the aerodynamic reaction, but it is necessary to average these with regard to time throughout one cycle in order to find if the weight and resistance can be balanced.

By formulæ 3 and 4 the lift is

$$KAU^2 \cos\beta \sin(\theta - \beta)$$

which, noting that $v$ varies harmonically, is equal to

$$L = KA\left\{V^2 + v_1^2 \sin^2\left(\frac{2\pi}{\tau}t\right)\right\}\cos\left[3^\circ + \sin\frac{2\pi}{\tau}t\left\{\tan^{-1}\left(\frac{v^1}{V}\right) - 3^\circ\right\}\right]$$

$$\times \sin\left\{\tan^{-1}\left\{\frac{v^1 \sin\left(\frac{2\pi}{\tau}t\right)}{V}\right\} - \left[3^\circ + \sin\left(\frac{2\pi}{\tau}t\right)\left\{\tan^{-1}\left(\frac{v^1}{V}\right) - 3^\circ\right\}\right]\right\} \quad (14)$$

and a similar substitution may be made for $T_1$ the thrust.

These forms do not appear to be integrable, but from the nature of the problem they can have no appreciable value during the first half of the cycle.

To apply them to any particular case they must be calculated for several positions during the downstroke, plotted to a time base and their average value so obtained. Let these averages value $L_0$ and $T_0$ for the half cycle. Then the averages for the whole cycle will be $L_0/2$ and $T_0/2$, and for steady flight we have

$$L_0 = 2W \quad . \quad . \quad . \quad . \quad . \quad . \quad (15)$$

$$T_0 = 2R \quad . \quad . \quad . \quad . \quad . \quad . \quad (16)$$

Roughly these quantities will vary harmonically through the hemi-cycle of the downstroke, for the difference between $\theta$ and $\beta$ varies harmonically on account of the simultaneous harmonic variation in these quantities. The resolution of the vector with regard to $\beta$ will, however, prevent this from being exact, as well as the initial value of $\beta$, and the variation in the relative velocity.

If we however assume a harmonic variation in the lift and thrust then the averages throughout the whole cycle will be $1/\pi$ times the maximum values.

The maximum values are

$$L_{max} = KA(v_1^2 + V^2)\sin 6^\circ \cos\left(\tan^{-1}\frac{v_1}{V} - 6^\circ\right) \quad . \quad . \quad (17)$$

$$T_{max} = KA(v_1^2 + V^2)\sin 6^\circ \sin\left(\tan^{-1}\frac{v_1}{V} - 6^\circ\right) \quad . \quad . \quad (18)$$

As a very rough approximation we might say that

$$L_{max} = \pi W$$
$$T_{max} = \pi R$$

for steady flight.

As regards the possibility of constructing machines on the ornithopteric principle, it is fairly clear that the chief difficulties lie in the question of timing and adjusting the angle of attack, and I hope the above discussion of the aerodynamic action may render considerable assistance in this direction.

## APPENDIX II.

There is undoubtedly some particular form of wing which will be most efficient under given conditions of beat and twist; with a given wing there must be some special relation of twist to beat which gives the best results. From analogous cases in machinery it may be safely concluded that the variation of the thrust and lift from mean values should be as small as possible (except in so far as "virtual mass" may be valuable) and that the quantity of fluid handled should be a maximum. It is also fairly certain that there can be only one speed of horizontal translation for any one wing which will give highly efficient working. Increased rates of reciprocation may, of course, render it possible to travel at a higher speed, but the normal conditions should be those of maximum efficiency. In addition it would seem desirable that this "normal speed" should be the same as the normal gliding speed considering the machine as an aeroplane so that easy transition from flapping to gliding can occur.

## APPENDIX III.

### LORD RAYLEIGH'S EXPOSITION OF FITZGERALD'S THEORY OF FLAPPING FLIGHT.

["Manchester Memoirs," Vol. XLIV. (1899), No. 5, pp. 24-5-6.]

"We denote by $u$ the horizontal velocity of the plane supposed uniform, by $v$ the vertical velocity at time $t$, by $\theta$ the inclination of the plane to the horizon at time $t$, while $S$ and $W$ denote the area and weight. If we assume the formula for the pressure, although the application is now to an *unsteady* motion, and further suppose that $v/u$ and $\theta$ are always small, we get for the whole normal pressure upon the plane at time $t$

$$KS\,(u^2 + v^2)\,(\theta + v/u) \quad . \quad . \quad . \quad . \quad . \quad (40)$$

in which, however, $v^2$ in $(u^2 + v^2)$ may be omitted.

We now assume that $\theta$ and $v$ are periodic, for example, that

$$\theta = \theta_0 + \theta_1 \cos pt \quad . \quad . \quad . \quad . \quad . \quad (41)$$
$$v/u = \beta \cos (pt + \epsilon) \quad . \quad . \quad . \quad . \quad . \quad (42)$$

where the periodic time $\tau$ is related to $p$ according to

$$\tau = 2\pi/p$$

At this stage criticism may present itself that the assumed motion involves a reaction for which we have made no provision. In practice the reaction is

supplied by the inertia of the body of the bird to which the wings are attached. The difficulty would be got over by supposing that there are several planes executing similar movements, but in different phases regularly disposed. It seems hardly worth while to complicate the present investigation by introducing a vertical movement of the weight.

By (40) the whole pressure at time $t$, perpendicular to the plane is

$$KSu^2 \{ \theta_0 + \theta_1 \cos pt + \beta \cos (pt + \epsilon) \} \quad . \quad . \quad . \quad (43)$$

Of this the mean value is to be equated to the weight $W$ supported, so that

$$W = KSu^2\theta_0 \quad . \quad . \quad . \quad . \quad . \quad (44)$$

The horizontal component of the whole pressure at time $t$ is

$$S \ . \ Ku^2 \{ \theta + v/u \} \theta \quad . \quad . \quad . \quad . \quad (45)$$

and of this the mean value is to be supposed to be zero, in order that the plane may move with uniform horizontal velocity.

Thus

$$\theta_0^2 + \tfrac{1}{2}\theta_1^2 + \tfrac{1}{2}\beta\theta_1 \cos \epsilon = 0 \quad . \quad . \quad . \quad . \quad (46)$$

Again, if $WU$ be the (mean) rate of expenditure of work

$$WU = S \ . \ Ku^2 \int_0^\tau (\theta + v/u) \, vd \, (t/\tau) = S \ . \ \tfrac{1}{2}Ku^3 (\beta\theta_1 \cos \epsilon + \beta^2)$$

$$. \quad . \quad . \quad . \quad . \quad . \quad (47)$$

If we eliminate $\beta$ between (46) and (47), we get

$$WU = S \ . \ \tfrac{1}{2}Ku^3 \frac{(2\theta_0^2 + \theta_1^2)(2\theta_0^2 + \theta_1^2 \sin^2\epsilon)}{\theta_1^2 \cos^2\epsilon} \quad . \quad . \quad (48)$$

from which we see that if $\theta_1$ be given (as well as $SWu$), $U$ is least when $\epsilon = 0$, viz., when the phase of maximum vertical velocity coincides with the phase of greatest inclination. In this case, by the use of (44), we have

$$U = (W/KSu)(1 + 2\theta_0^2/\theta_1^2) \quad . \quad . \quad . \quad . \quad (49)$$

If we regard $WSu$ as given, the smallest value of $U$ corresponds to $\theta_1$ being large in comparison with $\theta_0$ which is given by (44).

(It must not be forgotten that $\theta_1$ itself has been assumed to be small.)

The smallest value is

$$U = W/KSu \quad . \quad . \quad . \quad . \quad . \quad (50)$$

The work required to be done is here the same function of $SW$ and the horizontal velocity as was found in the case of the aeroplane.

We see from (46) that, under the circumstances supposed, $\theta_1 + \beta$ is numerically small in comparison with $\theta_0$, and *a fortiori* in comparison with $\theta_1$. Accordingly the forward edge of the plane is inclined downwards when the motion of the plane is downwards.

As regards the pressure, it is by (43) proportional to

$$\theta_0 + (\theta_1 + \beta) \cos pt$$

in which the second term is relatively small. The pressure acts always upon the under side of the plane, and the weight is approximately supported in all phases."

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[The only serious comment which can be made on the above remarkable analysis is that (1) friction should not be neglected, and (2) in practice the maximum value of $v$ is quite comparable with $u$ so that the approximations assumed need to be modified as has been done in the numerical example worked out in my paper.—H.C.]

## REVIEW.

**Non-Ferrous and Organic Materials.** Arthur W. Judge. Sir Isaac Pitman and Sons, Ltd. 25s.

This book will be found a useful one for reference. It is largely a compilation of published data rather than an account for personal experience. It contains much useful data, but it covers rather too wide a field to adequately deal with some of the subjects discussed. It will, however, be found useful as giving an idea of the properties of the various materials. The earlier chapters deal with non-ferrous metals. Chapter V. contains a useful and effective account of the classification and examination of different types of timber, whilst Chapter VI. gives an account of the tests to be applied to timber for the purpose of ascertaining its suitability for specific purposes. Chapter VII. is devoted to a description of tests applied to aircraft fabrics and coverings. The subject of dopes and varnishes for use in aircraft construction is dealt with as well as the space in Chapter VIII. permits. The handling of the chemical side of the preparation of dopes and varnishes is scanty. Chapters IX. and X. deal with glues and rubber in a useful manner. In the 27 pages of Chapter XI., devoted to paints and painting, the Author has succeeded in giving a useful resumé of the properties of pigments, oils and dryers and the application of paints to surfaces.

A general impression left on the mind of the reader is that the Author has succeeded in producing a book containing useful practical information to those interested in this branch of industry. The large scope of the book further necessarily results in a lack of detail which is pronounced in certain parts, but the illustrations and tables of data are effective in making clear the matter presented in the volume.